Tuesday, 27 September 2011

(9.30 am)

THE CHAIRMAN: Good morning, ladies and gentlemen. I think it's appropriate for the Inquiry to take note of the death of Frank Maguire. As is generally known, he was particularly active in the background -- I'll call them negotiations, but the background events that led to the setting up of this Inquiry and I think it's a matter of regret that someone who had worked so hard and so long to achieve this result should not survive to see the exercise completed.

It is not appropriate for me to say very much more than that, but, Mr Di Rollo, you might like to say something yourself to note the event.

MR DI ROLLO: Thank you, sir.

There have been many tributes paid to Frank, not just from within the profession but well beyond. It is obvious from these just how highly regarded he was and what a tremendous contribution he made in so many areas.

Many lawyers come across injustice, unfairness and deprivation regularly in the course of their working lives. It is one thing to notice such things but beyond the individual case, it is quite another to do something about them. Frank did. Few have the inclination, the talent or the energy to synthesize disparate voices of

dissent and mould them into a unified and effective force to challenge the received wisdom. Frank did.

Few have the necessary advocacy and political skills to influence decision-makers to secure necessary legislative changes. Frank did.

Few have the persistence and singularity of purpose to fight for those that would otherwise be ignored. Frank did.

His significant contribution to the setting up of this Inquiry is just one of his many achievements.

I do know how incredibly impressed he was by the preliminary report and the power of work undertaken by the Inquiry team. For those of us continuing to assist this process on behalf of the patients, relatives and the Haemophilia Society, we will try to do so in a manner fitting to his memory.

Thank you, sir.

THE CHAIRMAN: Thank you very much. I think it might be appropriate to have that extended and made available to his family. So take steps to have that done.

Ms Dunlop?

MS DUNLOP: Thank you, sir.

Before beginning with our witness for today, who is Professor Cash, it may be helpful to say a few words about this particular topic, which we have designated

B4.

Plainly it relates to the introduction of screening of donated blood for what later became known as the HIV virus. What I hope to do in the evidence this week is to fill in 1984 from the point where the discovery of the virus was announced in America, and also to deal with events in 1985, leading up to the introduction of testing in October.

Some similar issues will arise at a later stage in the Inquiry in relation to the screening of donated blood for Hepatitis C, where there was a longer gap between the availability of tests and their introduction in the United Kingdom. In relation to both viruses, on the matter of screening, there was a lot of work done jointly between Scotland and England and it is, I suggest, of interest to the Inquiry to look at that, as we see it.

I thought it might be useful, before Professor Cash gives evidence today, to give a short narrative of some of the events in 1984.

We have mentioned on a number of previous occasions the topic of the identification of the virus causing AIDS. For reference, the matter is dealt with in the preliminary report at paragraphs 8.81 to 8.89. We should, I think, have a brief look at that, just now.

The reference is [LIT0012479] at page 26.

For people with hard copies, that's page 211, as we can see on the screen.

There is, on this page, reference to the discovery of LAV in Paris in 1983 and the work of Montagnier and Barre-Sinoussi and then --

THE CHAIRMAN: Could I interrupt just a moment.

Professor Cash is not here yet, is he?

MS DUNLOP: I gather he is on his way.

THE CHAIRMAN: Yes, I wonder if we can make arrangements to ensure that he comes in as soon as he is available, so that he can hear as much of this as possible. I don't think that had been done, Ms Dunlop.

MS DUNLOP: I appreciate that, sir.

THE CHAIRMAN: I think just bring him in whenever --

MS DUNLOP: Yes, thank you, sir.

THE CHAIRMAN: Sorry, Ms Dunlop.

MS DUNLOP: Yes, I was just, I think, refreshing memories about this small section of the preliminary report. We go on, on 8.82, to talk about the naming of the French virus and then in 8.84 the press conference in Washington on 23 April 1984. We say:

"Controversy, which is not of immediate relevance to this Inquiry, developed over the discovery and its attribution."

THE CHAIRMAN: I hope that remains a valid comment, Ms Dunlop.

MS DUNLOP: Then we go on to talk about some of the events in the ensuing months. Perhaps it's worth noting also 8.88 on the following page, the reference to the announcement in the journal "Nature", on 14 June 1984, the announcement that the company selected by the federal government in America to manufacture a blood test for AIDS was expected to be announced that week.

Then also the development of a laboratory test for AIDS by Dr Tedder and Dr Weiss.

Then, of course, there is a further section in the preliminary report on the introduction of screening from 8.122 onwards. But I don't need to go to that.

To begin actually in February 1984 -- we have now ascertained that it was the laboratory of the now Professor Weiss, Robin Weiss, in London, was the first in the United Kingdom to become involved in investigating HIV. I should say at this point, sir, that I propose just to call it "HIV". I know there are naming difficulties and officially "HIV" was not used until 1986, but I think it's simpler just to call it "HIV".

THE CHAIRMAN: I think we have got the same problem with Hepatitis C, but it may be rather more difficult to

avoid distinguishing the two there, since I think that knowledge of what it was and the naming --

MS DUNLOP: I think all I'm trying to do is obviate the need for people to correct me and say, "It wasn't called 'HIV' in 1984," because I do know that.

THE CHAIRMAN: I think that I would have assumed that you knew it.

MS DUNLOP: Thank you, sir.

Anyway, to go back to what happened in February, Professor Weiss has told us that he received some French virus from Luc Montagnier and we can see that if we look at his letter, which is [PEN0171261]. Can we go to page 3, please?

This is his letter of 12 September to the Inquiry team. Under subheading "Final Comments", we see that he tells us that:

"During 1984 and 1985, virological research into HIV was just getting into gear. My laboratory was the first in the UK to become involved in investigating HIV, when we received the French HIV isolate, LAV, from Luc Montagnier in February 1984."

Obviously we will be going back to that because we are lucky enough to have a video conference with Professor Weiss arranged for this afternoon. On 23 April, as we saw in the preliminary report,

Margaret Heckler, the United States Secretary of State for Health and Human Services, announced that the probable cause of AIDS had been found, a variant of a known human cancer virus, called "HTLV-III". She says:

"Credit must go to our eminent Dr Robert Gallo who directed the research that produced this discovery."

We can see reference to the press conference in Douglas Starr's book. I would like to look at that briefly, please. That's [LIT0012936]. Could we go to the final page of that chapter, please? This is page 298 in the book.

In that paragraph beginning:

"It was science, finally ..."

We can see the reference to the press conference. Worthy of note at this stage are Margaret Heckler's comments, particularly in relation to testing. She said:

"Today we add another miracle to the long honour roll of American medicine and science."

Then she said:

"A test to screen the blood supply with 100 per cent certainty will become widely available within six months."

It is perhaps also interesting to note, although

it's not quoted in that paragraph, that she said it was hoped there would be a vaccine ready for testing in about two years. Within days companies were lining up to apply for a licence to make the Gallo test on a large scale.

Can we look, please, at the Crewdson book. This is the Crewdson book, Science Fictions, [PEN0170568]. It we go to page 4 in this particular extract, please.

There is what I would suggest is some helpful narrative here from this period about the Reagan administration's priority of putting the Gallo blood test into service. Just allow a moment for everyone to read that passage.

(Pause)

We have, of course, become familiar with the names of certain pharmaceutical companies. So it's of some interest to note that the first to apply was Baxter Travenol.

THE CHAIRMAN: Some others that we haven't heard of towards the bottom.

MS DUNLOP: Yes. Biotech boutiques, I see referred to. Chiron coming in. Also on the list, and this is further down the page, were the three Gallo contract companies who had helped to grow the AIDS virus and perfect the blood test, Biotech, Electronucleonics and

Litton Bionetics. Then, interestingly, at the foot of the page:

"The most formidable competitor by far was Chicago's Abbott Laboratories, which had pioneered the field of diagnostic blood testing and dominated the worldwide market. Although Abbott's consumer items were household names, the bulk of Abbott's business was done with hospitals and laboratories, to many of which Abbott already was selling an ELISA for antibodies to the Hepatitis B virus.

"Racing to meet Heckler's six-month deadline, an HHS committee charged with evaluating the competing applications chose Abbott first and Baxter-Travenol second, followed by the three Gallo contractors -- but only after Electronucleonics and Biotech had formed hasty commercial partnerships with Organon and DuPont to manufacture and distribute their tests."

Some more narrative about the background to the granting of the licensing applications. Worth noting towards the foot of the screen is that:

"No AIDS test could reach the market until its quality had been verified by the US Food and Drug Administration, the agency charged with approving all medical devices. The real reason for the patent licences, an HSS lawyer acknowledged, was to give the

five chosen companies the opportunity to develop a strong market position."

We then see that there were royalties going to the Department of Health and Human Services, but according to the author:

"The real benefit for the government, one HHS attorney admitted, was scientific pride."

Abbott we see described here as a "gargantuan bureaucracy":

"But the head of the company's diagnostics division, Jack Schuler, intended to beat the smaller companies at their own game by creating a small company inside a large one. To head Abbott's ELISA team, Schuler chose a 28 year-old researcher and gave him ten people. Schuler's only instructions were that Abbott must be first to win FDA approval. 'I told them nobody is going to remember who was second,' Schuler said."

We can see a certain demonstration of commitment in the fact that when the companies gathered in Frederick to pick up the virus isolate, the Abbott contingent arrived in one of the company's fleet of corporate jets.

If we go further down that page, we can see that scientists eager to begin work on a cure or a vaccine for AIDS, were all being given one particular isolate. That was the one that Dr Gallo called "HTLV-III B":

"'We received the HTLV-III B isolate in May of 1984,' recalled Robin Weiss."

We should note as well that, Dr Weiss who already had a French isolate, received an American one in May 1984. Then on 14 June there was that publication in the magazine, Nature, reporting on the licensing of the companies. [SGH0026605]. Much the same information. We can see there is a degree of underlining in this piece, including the fact that millions of test kits will be required each year to screen blood products and a mention of ELISA techniques.

Genentech had submitted a proposal through its joint venture with Travenol. Genentech-Travenol Diagnostics. In the light of that we can see a memo that was sent within the DHSS, [DHF0015506], dated 25 June.

My hypothesis, sir, would be that this might have been written by Dr Oliver. I say that only because, everyone may recall from the evidence about leaflets, we do have a document which, along the bottom, lists who was in what room. So if he was still in the same room, it would be Dr Oliver.

THE CHAIRMAN: Any guesses as to who they discussed the problem with, blank, when we meet "him", might we?

MS DUNLOP: I suspect, sir, we will come --

THE CHAIRMAN: I'm sure that there is a reason for it.

I just wondered if you have any --

MS DUNLOP: I'm not sure what name would go in there. It might be Dr Gunson because Dr Gunson was holding a meeting at the end of June 1984. I should say, as will become apparent during the day, we are in the course of receiving what one might call de-redacted documents, in other words, documents from the Department of Health, where I think some of the names have been filled in, and that's very helpful and it may be that this document will be filled in in due course.

THE CHAIRMAN: The only real interest is in the level of officer who would be involved at this stage, I think.

MS DUNLOP: Yes. We can see two questions are posed. Firstly:

"Do we foresee doing the test routinely on donor blood, and if so would we have to buy US kits to do it?"

There is then that ad hoc meeting in London, which was on 28 June 1984, and I intend to revert to that. It was a meeting to which Dr McClelland was invited but, because of a train strike, wasn't able to get there.

Then 3 July. Can we look at another memo, [SNB00065978]. It's not a memo, it's Dr Gunson's letter to Dr Alison Smithies, and it's written in the aftermath of the meeting. Dr Gunson is reporting the meeting on 28 June with Dr David Tyrrell, Dr Richard Tedder,

Dr Wallington, an immunologist from Bristol Blood Transfusion Service, and Dr Marcella Contreras, who I think was from the North London Blood Transfusion Service at Edgware. We can see that one of the main topics has been the whole question of screening tests. Dr Gunson says in paragraph 4:

"One of the major problems is that considerable pressure will be put on the transfusion service if this test is introduced in the USA. However, we all agreed that at the present time this test should be regarded as a research project and that it should not be introduced as a routine screening test on blood donations without proper appraisal."

Then in a section underlined:

"The most important development in the study is the availability of a viable test for anti HTLV-III."

So HTLV-III antibodies:

"In this regard, the work of Dr Robin Weiss of the Chester Beatty Institute in cooperation with Richard Tedder is very promising ... there is some reason to believe that a radio-immunoassay may be available within the foreseeable future."

A general protocol has been agreed for some study, using this test, and we can see that on the next page, three stages set out. Once the test is available donor

serum samples would be sent from the Northwest Thames Regional Transfusion Centre to the Middlesex Hospital;

"... where the performance of the test will be proved and evaluated.

"Stage II. The performance of the test will be transferred to [Northwest] Thames RTC ...

"Stage III. Donations will be tested at the Manchester RTC and the Bristol RTC while tests will continue at [Northwest] Thames."

Looking from this exercise to get a broad view of the country as a whole.

THE CHAIRMAN: I don't know how far you are going to follow this but I find the particular letter interesting in a number of respects. One of them is the contrast between the development of a protocol for screening donations on the one hand, and the decision simply to go ahead and test patients on the other. But perhaps you will deal with that in due course. It's the structure of the letter that I found very interesting, when I got down to studying it.

MS DUNLOP: There is another passage which we should note at this stage in the letter, in the paragraph which we can see at the foot of the screen. The passage I'm highlighting begins with the words, "When the reagents are available." Dr Gunson says:

"It will be necessary to make test kits. It would be an advantage for the NBTS if this was in the format of the BPL RIA test for Hepatitis B surface antigen, and this concept is being considered by Richard Tedder at present. We briefly discussed the possible role of the CBLA in the preparation of the test kits, and whilst this is an option which may be available, there are others, such as collaboration with industry, which will have to be considered.

"I have written at length about the possibilities of developing the test in the UK, since the alternative will be to purchase kits from an American company such as Abbott Laboratories. I dread to think what the cost to the NHS will be under these circumstances."

Then he goes on to mention a need for additional staff, possibly additional space. Then in conclusion on the final page, he describe this is as a major decision. We can see that the letter was copied to Dr McClelland.

Then on 16 July, more concern really about the implications of these developments. [DHF0029126]. This is a letter to the DHSS, I think possibly to Dr Smithies, from, I suspect, Dr Ian Fraser of the Bristol Blood Transfusion Service. If we look at the heading, please, for a moment. We can see it's coming from NBTS in Bristol. And there is the reference to

IDF. So I think it is Dr Ian Fraser. If we go down through the letter, we can see the same topic being discussed.

I should add, sir, that I think Dr Fraser was the chairman of the NBTS directors at this point. So not just writing in his capacity as director at Bristol but with a wider remit. He says:

"A screening test for AIDS is likely to be available within the next eight weeks or so, for trial at Edgware and then at Bristol and Manchester."

I would like also to highlight that sentence in the middle of the larger paragraph:

"It was also felt that once it is known that the screening test for AIDS is available, many members of the public may turn up at blood donor sessions in order to have this test carried out. The Public Health Laboratory Service, however, will also wish to evaluate this test and make it available to the 52 PHLS laboratories in the country."

So just to note that the concern about people using blood donation as a mechanism for obtaining an AIDS test has been identified by this point in July 1984. We see at the bottom of the page concern also about cost. We heard that the cost per test could vary between 20p and £5. And then onto the next page, please.

THE CHAIRMAN: Professor Cash has arrived.

MS DUNLOP: Yes.

PROFESSOR JOHN CASH (continued)

THE CHAIRMAN: Good morning, Professor Cash. We are having a preliminary run through some documents, which should be of interest to you.

Questions by MS DUNLOP (continued)

MS DUNLOP: Yes. A brief recapitulation, perhaps, Professor Cash.

We have looked at Dr Robin Weiss obtaining French isolate in February 1984 and then the Gallo isolate in May 1984. We have looked at the Margaret Heckler/Robert Gallo press conference in Washington on 23 April 1984, at which various announcements were made about how quickly a test would be available and indeed a vaccine too, the licensing of the five companies which were working on developing tests in the United States, the occurrence of an ad hoc meeting at the end of June 1984 in London. That was the meeting to which Dr McClelland was invited but couldn't get there because of a train strike. Now we are in July 1984, looking at what was happening and the fact that a British test, an RIA, was eagerly anticipated at this point, but there were concerns about cost, concerns really about how to implement testing and also

identification of the possibility that members of the public would start using blood donation as a way of obtaining an AIDS test, which is something you cover in your statement.

So we are looking at this letter, which we are pretty sure comes from Dr Ian Fraser, who was the director in Bristol, and also at this point the chairman of the NBTS directors.

A. That's correct.

Q. This is his letter to the DHSS. Advocating the urgent setting up of a working party on AIDS:

"... as when the screening test for this disease is generally available, there will be numerous problems to sort out."

Perhaps we could go back to the first page to let Professor Cash have a quick look at that too.

A. Thank you. Could you remind me of the date roughly?

Q. This is 16 July 1984, yes?

A. Thank you. Yes.

Q. We move now to 27 July, please. [DHF0015554]. This is another DHSS minute:

"There have been further developments regarding the radioimmunoassay for antibody to HTLV-III."

So we can deduce that it's now available. So that's the British RIA, which in fact had been developed from

the Gallo isolate, it's now available:

"Some 2,000 tests have been carried out on AIDS patients, patients with the extended lymphadenopathy syndrome, homosexuals attending STD clinics, haemophiliacs and others."

I think, in fact, including ordinary blood donors, if one can call them that.

Can we look at the next document, please, which is a paper setting out some of the many problems, it says. That's [DHF0015555]. Numerous problems thrown up as a result of recent developments following the identification of a causal virus. It's proposed that a working group should be set up to provide this advice. Can we look particularly, please, at the next page, page 2, and go to section 4. I think no particular mystery about that sentence. It must be:

"By collaboration with [Dr Gallo] in the United States and [Luc Montagnier, Dr Montagnier,] in France ... "

It would be, Dr Weiss and Dr Tedder:

" ... have obtained isolates of the causal virus. They expect to isolate a similar agent in the UK shortly ... they have been able to devise a test which uses a radioimmunoassay technique to identify antibody to HTLV-III virus in the blood of AIDS patients."

We see again a plan to extend the test to blood donors at the North London Transfusion Centre:

"To ascertain the prevalence of positive cases in the blood donor population, the sensitivity, specificity and practicality of the test for blood donors will also be assessed."

Mention of a need for money, unsurprisingly. Then section 5, that there is going to be pressure, the writer anticipates, to institute the use of the test in all regional transfusion centres and to extend its use beyond that. The UK test is found to be accurate, there will be a need to scale up the production of the reagent further.

Then can we look on to the next page to section 6, please. A note of a need for clearance from the United States to permit use of the American isolate for any extended production of the reagent. The French agent is currently difficult to propagate. Then this 20p, £5 dilemma:

"It is currently estimated that the test reagent developed by [Tedder, I think that must be] and Weiss will cost 20p per test, whereas reagents likely to be produced by USA pharmaceutical companies, five of whom have been given the isolate to develop, may cost up to £5 per test."

Then on to the last page, please. Mention again of the need for a group to look at the issues:

"It is suggested ..."

This is at the very end of paragraph 10:

"... that an expert group should be drawn together to consider the problems and provide guidance, informing such a group the interests of the whole of the United Kingdom should be kept in mind."

On 10 August a letter was sent from the DHSS to America to ask about using the isolate, and that's [DHF0015619]. We can see that going to the assistant secretary for health in Washington. It is possible that this letter was sent by Dr Walford. We can see that this is seeking permission for the use of the test, which Messrs Tedder and Weiss have developed, within the NHS. The author -- I'm just going to call her "she" -- says at the end of the second paragraph:

"A paper is shortly to be published in the Lancet in which the authors describe both the results and details of the test itself ..."

That, sir, is the Cheingsong-Popov paper, which is published on 1 September 1984. She goes on to give some details about the AIDS situation in the United Kingdom, and then at the bottom she says:

"I'm writing to request your agreement to our using

the virus isolate originally provided by Dr Gallo to scale up production of the antigen. I hope that you would be able to look upon this request sympathetically. It will enable us to establish knowledge of the epidemiology of the condition in the United Kingdom more rapidly than would be the case if the test had to be developed from our own isolate, and it will of course contribute to the universal need to know more about this disease.

"I should stress that the screening test developed from this isolate would be used mainly by the National Blood Transfusion Service within the National Health Service. As you know, the NHS is a non-profit-making, public sector body."

We know, sir, from subsequent documents that an unhelpful response was finally received on 14 November 1984. I have not been able to find that. I think it's possible that we simply don't have it but we know enough of the other circumstances to know that the response was indeed unhelpful, and we also have a letter, which we don't need to go to but I'll give the reference for it, in December to the Chester Beatty Laboratories, [DHF0018858], which seems to be reinforcing the point that the test can be used for scientific research or sampling work.

The story is told in more detail in the Crewdson book. Can we go back to that, please, [PEN0170568], and goes to page 25? We can see there the information about the British AIDS tests.

I think this is wrong in saying that it was an ELISA because from everything we have seen it was an RIA not an ELISA, but we can ask Professor Weiss about it this afternoon. I hope the rest of it is accurate.

A. Could you remind me, this publication that I'm looking at now...

Q. This is the John Crewdson book, "Science Fictions"?

A. Oh, yes.

Q. I think John Crewdson of the Chicago Tribune.

A. Thank you.

Q. We can see what bear to be some quotes from the time, and a snippet of information about a Daily Telegraph reader sending a copy of the article to President Reagan. No British company had applied for a licence to sell the Gallo test, I think this is.

Yes. We can see certain intellectual property considerations featuring there. In this sequence of documents, the next one I want to look at is dated 13 August 1984 and it's another Department of Health and Social Security memo. It's [DHF0025897].

It's a memo to Dr Harris and it's back to the topic

of a proposed working group. It mentions the recent development of the RIA. We can see at the end of the second paragraph, a comment which we have tried to investigate:

"We would therefore be in a strong position to make decisions about the need to buy from one of the five US pharmaceutical companies who have been licensed to produce a screening test and are likely to wish to start marketing these tests in the UK in the next few months."

Sir, the word "avoid" isn't used in the sentence but it seems to be the sense of it, that the writer is perhaps hoping that the need to buy from the American companies may be avoided if a successful British test can be produced. Then into paragraph 4:

"It is considered these issues primarily concern the National Blood Transfusion Service, although there is an overlap into the interests of policy divisions concerned with AIDS as a communicable disease."

Then a working group of the advisory committee on the National Blood Transfusion Service would be an appropriate forum, and suggested terms of reference.

Then if we can look at the other side, please, and there we have it, a de-redacted document. Many of the names in fact being now quite familiar.

THE CHAIRMAN: Just a couple who are not particularly

familiar. Dr Rodin and Professor Mitchell, a different Mitchell from the one we are familiar with.

MS DUNLOP: Yes, and we see that observers were to be invited from the army, the SHHD, the Welsh Office and Northern Ireland, and Dr Abrams, senior principal medical officer, is to chair the group.

Wanting the first meeting as soon as possible, if we look a little bit further down. I think this is going to say "Alison Smithies"? Yes, it's a memo from Dr Alison Smithies, who by this point is actually in Dr Walford's old room, for those who follow these developments.

There is a memo also saying that Dr Smithies took over Dr Walford's duties. Then on 1 September, as I have already said, the Cheingsong-Popov paper was published in the Lancet. The reference, although I'm not going to it, is [LIT0010417]. Another memorandum from 25 September 1984, [DHF0015746].

The only part I really wanted to focus on, because this is a more general update on AIDS, is the end of section 2, where we can see the results of testing tabulated, and a comment that:

"The competitive RIA and immuno-fluorescent techniques employed by Weiss, Tedder and their colleagues are among the most sensitive and specific

tests available."

Then from the next day, [SGF0010929], we can see that Dr Bell wrote to Dr Abrams on 26 October, referring to a conversation between Dr McIntyre and Dr Abrams concerning:

"... the implications of this group's work for SHHD and the SNBTS, as well as DHSS and the NBTS."

He says:

"I understand that you are willing to extend the expert membership accordingly and the SHHD is nominating Dr Brian McClelland."

Dr Bell goes on to say that he will be glad to attend on behalf of SHHD. So, as at the end of September, the intention is for Dr McClelland and Dr Bell to go to this working group.

Everything else, I hope, of relevance we are going to be covering with Professor Cash or one of the other witnesses. So with that introductory narrative to rehearse the early months of the period, I would like to turn, please, to Professor Cash's statement, which is [PEN0171038].

Professor Cash, I haven't welcomed you back. I should welcome you back. Thank you for returning so soon to tell us again about a different topic, which obviously is the question of screening in relation to

AIDS.

A. Can I apologise for being late, sir. I had two letters and the only difference between the two seemed to be the word "October". I'm due here on 27 October at 11 o'clock and I am afraid I mixed them up. I do apologise.

THE CHAIRMAN: I wouldn't be surprised if they want you back then too, Professor Cash.

MS DUNLOP: I think we do.

THE CHAIRMAN: But there is no concern.

A. I have no minders these days.

PROFESSOR JAMES: Except your wife.

A. No, she has thrown in the towel, sir.

MS DUNLOP: You have, for us, professor, helpfully provided some background notes and, as you go on to say, many of your responses to our questions are actually contained in the background notes. So we need to look at them.

You tell us that:

"It has always been [your] view that the overall management of the UK Blood Transfusion Service's responses to HIV/AIDS was a matter of some concern."

We need to look at the first of those references, which is a letter, [SNB0057304]. You instance this, I think, in your statement as one of the letters in which you recorded concern at the time. And we can see

that it's a strictly confidential missive that you sent to Dr Bell.

We can see from the second paragraph that you are concerned about reports you have received from those who attended the meeting of the working group. So the working group, whose establishment we have just been looking at, by this point it has met.

We will come on to look at that in more detail, but it met at the end of November 1984 and there was a meeting of SNBTS directors on 11 December, at which Dr McClelland reported that he had found it a disappointing meeting. He advised his fellow directors of a decision at the meeting, however, that all donors should be tested once the test was available.

Disappointingly also though, no second meeting had been arranged and -- this is all paraphrasing from the minutes of 11 September meeting -- there appeared to be no co-ordination of the many splinter groups which existed.

So you are recording concerns about what you have heard of the meeting. Then you go on to remind Dr Bell about a meeting in February 1984, where you say:

"The question of AIDS and the transfusion services was discussed ... there was an urgent need for the topic to be taken up as a UK (joint departmental) matter."

You refer to your letter of 15 February 1984. We need to look at that. It's [SNB0048639].

Sorry, can we keep this letter open as well, because we will be going back to it. Thank you.

In fact, the letter is headed "Transfusion-associated AIDS Research", so if we look at this letter from February 1984 it does look, Professor Cash, as though you are conveying concerns of the SNBTS directors about a possible lack of co-ordination of AIDS research. Is that right?

A. Yes, but I think at that time -- I think there are different sorts of research. I wasn't thinking of any sort of fundamental biological work; I was looking at -- we laid them out lower down -- the practicalities. And I would describe these, if I may, as "applied research", in which we are doing stuff to find out what we can implement tomorrow, as it were.

I have looked at these papers in some detail and it is possible, but only just possible, that our colleagues in the department went down the track of research in a manner that -- but we will come to this no doubt -- which I wouldn't agree with. But this was about, this letter -- and I spoke to Bert Bell as well -- about getting the Department of Health to recognise that in terms of AIDS, the safety of our blood was to be

challenged, and we all needed to get into the boat together to actually look at how we could make sure that our blood was safe.

Q. Well, we can see what you are proposing; you are proposing a joint group and you are proposing, as you say, applied research, namely looking at prevalence really in homosexual males and a controlled blood donor population.

A. But the tests.

Q. And then also looking to see how good existing tests might be. That's in February 1984. And although we haven't gone into it, there were in fact some suggestions at an earlier stage about surrogate tests that might be of some value and so on.

A. Absolutely, Hepatitis B.

Q. Right. Can we go back to the January 1985 letter then, please? You return to the topic of the unsatisfactory nature of the November 1984 meeting of the working group. You say that:

"The Scots were there as observers. The SHHD representative had no experience in transfusion matters."

Professor Cash, I think you are meaning Dr Covell. Is that right?

A. Yes, indeed.

Q. I haven't yet got to the bottom of this but, as we will see when we look at the documents pertaining to the meeting in November 1984, Dr Bell's name was scored out and Dr Covell is written in in handwriting as the person who is coming from SHHD.

Quite why that change was made is the thing I haven't got to the bottom of, but we do know from Scottish Government, because we have asked them, that Dr Covell was a senior medical officer, whose areas of responsibility included communicable and sexually transmitted diseases.

So given that we saw earlier that there was a reference to there being some read-across into the STD area, we can see that his particular expertise may have been thought to have been relevant because of the wider implications in the sexually transmitted diseases forum.

A. Well, I would only comment by saying I'm reasonably certain that it was a surprise that we learned in early 1985 that Bert Bell was going to retire.

I think I have said on a previous occasion, despite the documents we looked at last time, for us he was a good friend and colleague. That's how we felt and I recall -- I'm fairly sure that by mid -- we held our dinner for him -- by mid 1985, and it is just possible in preparation of that the departmental colleagues

needed some -- rather than continue with Bert in this environment, they looked for somebody else. He was replaced in due course, was Bert, by John Forrester but that may not have quite taken place. So they were desperate that there was some medic there who had at least some interest in communicable diseases. From our point of view, he was not a transfusion man as Bert Bell very much was.

Q. I appreciate the distinction.

Can we look at the second page of the letter, please. You are actually referring at the top of this page to a kind of mirror-image meeting. That is the NBTS directors.

A. Yes.

Q. Who also found the meeting disappointing, and we should just look at their minutes, [SNB0111971], whilst keeping the letter open again, please, if we could.

In fact we can see you were there. This is the regional transfusion directors' meeting in Manchester, 23 January 1985. There you are, Dr Fraser, Dr Mitchell.

A. Dr Fraser was chairing it.

Q. I'm sorry?

A. I imagine Dr Fraser was chairing it.

Q. Yes. You were welcomed back after illness, we see, and then if we can scroll down. Firstly 4(d):

"The AIDS working party met in November. Dr Harris, Dr Lane, Dr Gunson, Dr Contreras, Professor Thom, Dr W B McClelland ... "

I'm not sure if that's right. Certainly Dr Brian McClelland had been there and he is not on the list.

A. That's the irony.

Q. Actually he was W M McClelland because he [the Northern Irish Director] was "Morris".

A. Indeed.

Q. So this is a hybrid. Professor Weiss, Dr Tedder, Dr Liddle and Dr Fraser:

"It was felt by RTDs in attendance that this was an unproductive meeting, there being as yet no new leaflet, no finance, and no positive move towards full donor screening."

Can we go also, please, to page 4. There is a whole section, section 7, entitled "AIDS". Interestingly in the fourth paragraph:

"Most companies are approaching regional transfusion directors."

These are ELISA tests:

"The preference within NBTS is for an RIA technique. Dr Gunson is to pass this information to the DHSS. The suggested cost is in the region of £2 per test. The

meeting felt strongly that we should not be pressurised by commercial sources to accept a test which is not ideal for our purposes, and that we should act together. The DHSS should be pressed to make any test available to the community before its use in blood donor screening, otherwise unsuitable donors will be attracted."

So it does appear that there is unanimity between the Scottish directors and the English directors at this point.

Can we go back to the letter, please? You refer to there having been a secret meeting. I'm not yet able to enlighten you from our references as to what the secret meeting was, but we may do better as the week goes on.

A. Have you, may I ask, contacted -- is he alive still? -- Richard Lane?

Q. We haven't asked Dr Lane but we do have both Professor Weiss and Dr Tedder. So we will ask them. We have information about Dr Lane. I don't think Dr Lane is in a position to attend.

A. It's worth knowing that the BPL -- that's Richard Lane's team -- had their own RIA kit for Hepatitis B surface antigen and it ran into some technical difficulties, you will not be surprised to know, picked up first by the guys in the West of Scotland, and that produced all sorts of problems. It was my understanding in the

period -- and I mentioned this the last time I was here. In the period I tried to befriend Richard very closely it became apparent that Wellcome again were handed -- in other words, the technology that was coming out of BPL, the kits for Hepatitis B surface antigen, was transferred, at my understanding, to Wellcome. I wasn't involved.

Lane and Gunson were very angry about the way this had been done. Stalking behind all this -- and Robin Weiss talks about it in his letter -- is the British Technology Group. I don't know whether you have researched that but that was a company that was formed with one shareholder, which was Her Majesty's Treasury, and this group's job was to, very laudably, get into the commercial domain research that was going on in academe or NHS or whatever.

So Lane and Gunson were very twitchy about the whole of this, and then came HIV, and this is the reason, one of the reasons, I thought that Gunson and Lane's proposition that we should go for RIA and it should be manufactured and released from BPL was not going to run, because the politics had moved on from there.

I think the secret meeting -- he addresses -- the secret meeting was probably -- but I'm not sure it was Robin Weiss was involved -- was Tedder, departmental

people and so on, as they began to think, "We cannot again have a kit for this important task manufactured in the public sector." There were quite strong political pressures about that.

So I think the secret meeting may have been secret, it wouldn't surprise me. You will find great difficulty in finding out what actually happened. What we know are the outcomes.

Q. Certainly, Professor Cash, you have mentioned two elements which we can spot in the documentation: one, the preference for the RIA -- and we have seen that from Dr Gunson and from others in English transfusion circles -- but also the suggestion that BPL might be involved in kit manufacture, which doesn't feature as prominently but certainly I have noticed it and we will see an instance --

A. If you look, you have got papers that the board of CBLA -- they were strongly supportive of this. The notion, forgive me, that it's going to cost 20p if we make it, or they make it, and £5 for the commercial ...

I mean, it's just nonsense. The fact of the matter is many years before, we, my own lab, had developed a technique, which is still rooted in the diagnosis of pulmonary embolism in the clinic, if you go into the Royal Infirmary today. We got overwhelmed with requests

from people all over the world to come and learn this and we handed it over for nothing, I might add, to Wellcome, and I remember getting very distressed to discover, when it finally came out -- I exaggerate no doubt -- it was £5 a test. We knew it was 20p for 100 tests.

I learned then about the notion of what the market will bear, and the moment you get into the commercial marketplace, it's a whole change(?) difference. There is evidence of that same phenomenon in the story that is now unfolding in terms of the marketability of Factor VIII; the price set for England in terms of their -- eventually underscored what the commercial boys could do. So the marketplace, this whole business of cost in terms of the kits, in terms of Factor VIII, in my view is a balloon. It's not real.

Q. Right.

A. Sorry to have gone on.

Q. No, no. We can see you rehearsing some of the same concerns in the next paragraph.

You are saying that the NBTS directors are aware that at least three foreign companies are actively involved in establishing proving trials for their HTLV-III antibody kits in UK transfusion centres, and that you are suggesting that they are doing that to

provide data for FDA licences, and once these licences are released the floodgates would open.

You and Dr McClelland both think that licences are going to be granted despite a possible 10 to 15 per cent incidence of false positives. All these would require sophisticated Western Blot check tests and the problems created would be significant.

So you are apprehensive about the activities of the American companies and you are worried about false positives, and we can understand the logic of that, that false positives -- obviously any positive has to be investigated and the donation can't be used. So these are significant problems which would have to be addressed by the transfusion service if a test had been introduced.

A. You would also have to the tell the donors, "Sorry, don't come back ever again", "But I haven't got AIDS?" "No, no, no, we don't think you have." But it would be "we think".

Q. I want to go on, because you have referred us to an article from Transfusion about some of the problems that occur in precisely that situation, and as I say, we can see the logic of that. Then you go on to say -- and this is in the final paragraph on this page:

"The biggest anxiety of the NBTS directors with

regard to this problem is the Scots: that they will unilaterally move to come in line with American proposals. They are right: we are in detailed discussion with commercial (kit) companies, our technical staff are already looking at ways of introducing the technology within existing staff establishments, we have the Western Blot technique, (HQ and SE Labs), we are already liaising with local (Communicable Disease) physicians with a view to securing care for our positive donors and we are currently arranging our financial planning accordingly ... "

Then you go on to say you have tried to reassure the NBTS directors that you would do everything possible to avoid such a development, but they haven't been entirely convinced.

It did seem to me, Professor Cash, that the point you are really making -- and this emerges from the last two paragraphs -- is dissatisfaction with the management of NBTS and that you are hoping that SHHD might be able to take some steps, I suppose, through government circles, to improve the management of NBTS by the DHSS. Is that a reasonable summary of your aim?

A. Yes, but I had been doing that since 1979. There are some interesting letters I wrote to Ed Harris, who was

the deputy chief medical officer, all about the whole question of management, and they didn't sort it out -- this was in 1979. It was finally sorted out in 1993, I think.

Q. Right.

A. But -- so there is no way, to be absolutely honest, as the HIV thing struck us that we could entertain complete reorganisation of our mates south of the border. That didn't come until 1991/1993. But what I was really wanting was to really get the seriously interested people, who are concerned about safety of blood, around the table and really working closely together, and we didn't need to get the English service reorganised to do that. We needed a DHSS that was committed to that notion, and that's really what I'm saying, really, to be honest.

Q. Could we go back to Professor Cash's statement, please. That's [PEN0171038].

We are still on the first page and we were looking at your references, which you have identified to show your concerns along the way with the way in which the Blood Transfusion Service response to AIDS was managed. If we look at the next letter, [SNB0132233], that's a letter of 12 February 1985 to Dr John Reid, then chief medical officer at SHHD. And you are writing in your

capacity as consultant adviser to SHHD in transfusion matters, I think. Is that right?

A. Yes.

Q. And you are saying that:

"Most of the emerging commercial kits may have a false positivity rate which, in the context of testing all blood donations, is embarassingly high."

You are saying:

"The introduction of this practice ..."

That, I suppose, is screening with these commercial kits?

A. Indeed.

Q. "... as of now, February 1985, would be ill-advised."

You make three points: distress and suffering for a number of donors and their families, the lack of counselling facilities and of appropriate technical back-up services to ascertain false positivity. Then your concern about loss of donated blood. So a reduction in usable donated blood. These are your three points. Then again you are disappointed that there has been, in your view, insufficient discussion by working groups and you think the demands of the marketplace are going to overwhelm the working groups. You think there is madness afoot.

Then you go on to advise, again, a three point plan.

You want a statement saying that the NHS is committed to the introduction of testing for all donations but that this will be actively discouraged until such times as the government is advised that its introduction is in the best interests of the transfusion services, and that active steps will be taken to establish a national kit evaluation programme.

What do you think was the madness that was afoot?

A. I'll give you a for-instance.

The first -- when we started finally screening, a large number of GPs panicked, to be honest, when they were faced with patients with HIV positivity, or donors were coming into them very anxious that they had been picked up and the BTS -- the counselling service didn't exist and the GPs had not been properly ...

I personally had a major battle and I was so delighted with the support I got from my medical colleagues in the department -- about the dental -- the dentist. There was a major crisis in Edinburgh, I knew for sure, about how did you get routine dental care for somebody who was query, query HIV positive? And that had not been thought through.

So the madness is that we didn't -- we had not sat down and thought the whole thing through. We had not, even by February -- I mean, we had come up with

proposals for looking at the tests and so on, which we will no doubt come to, much earlier on. They had not -- that had not been properly addressed. The commercial companies meanwhile -- I mean, there is a view that I see here, that there weren't many tests available, that this was a major problem. That never struck me.

There seemed to me to be a pack of companies hunting in this area and that we could get completely overwhelmed, ie we were being forced into something that we did not know as transfusion people, it had been left and parked aside, left to a DHSS expert group, which we will no doubt come to later.

We, the people responsible, were not in control of actually, urgently getting together, looking at these tests, to actually determine: is it 1 per cent or 0.5 per cent or 10 per cent? What are we dealing with here? For me, as a manager, it's a madness when the whole thing is drifting away here, and there are very serious matters that, in my view, were not being properly addressed.

Q. Right. I think, professor, we can see from the correspondence that you are making a number of points. You are certainly in favour of a national evaluation programme. You are in favour of the screening in principle.

A. Oh, yes.
Q. But --
A. But a national evaluation programme that gets on with it.
Q. Yes.
A. That actually goes for the bullseye, which doesn't start messing about as, in my view, they did for six months.
Q. I think this comes across too, that you do not want the introduction of kits which are going to cause more problems than they are going to solve.

Yes, you are nodding.

Can we go back to the statement, please? The other two references you mention I'm not going to go to for reasons of time. [SNB0112362] is a letter of 6 February 1986 to the then Dr Donald Acheson, chief medical officer, on the topic of money for AIDS research and proposing a subgroup of the Expert Advisory Group on AIDS to deal with this, and then [SGH0027524] is a letter of 7 January 1987 to Dr Ian Fraser discussing a number of familiar topics: leaflets, the management of National Blood Transfusion Service and the need for a joint approach. Then you go on to itemise five particular areas of concern. We are this week dealing mainly with C to E. You go on to tell us that you are going on provide some further background information on

topics C to E.

You pose a question, which we see in bold:

"Who had the duty of care with regard to the safety of blood and blood/plasma products in the UK?"

You say that wasn't clear. Might it have been all those involved, Professor Cash? Did you yourself analyse the position as being that those who made blood or plasma products would have a duty to take care with their part of the operation? Those who collect the blood, prepared it for direct transfusion would have a duty of care in relation to their part of the operation and then the government would be responsible for those parts in which it was involved? So it might not have been a question that admitted of a single answer.

A. I never even signalled that that was so, and indeed in 1988, in the Scottish Office, a meeting was called by the civil servants to discuss the potential for litigation that would emerge with HIV. This was a famous meeting in September 1988 and it was all about, it transpired, where in the NHS were the weak points in terms of any litigation in relation to this. And I produced an A4 list of areas that I thought were weak and one of them was the same question: who had the duty of care? And in my view it had always been a group of

us, depending on where you looked and so on, and that we needed therefore to get together -- this is 1988 -- we needed to have got together and we had not done that.

Even in my view, which -- my friends may not agree. I thought the haemophilia directors should have been around the same table in terms of duty of care, because in my view the haemophilia directors had a role to play in the exposure -- in the UK -- in the exposure of material that was pretty dangerous, and we can discuss all this together.

But we were unable, in my personal view, because DHSS/SHHD were not prepared to actually sit down and talk through this. I suspect it was a concern about funds and money again, which I was very sympathetic to. But I don't think that should have prevented us sitting down and really knocking the things around, to sort out as best we could.

I used to say that I used to feel I was in a boat and I was at the back end singing to my mates, the Scottish transfusion directors, "Come on boys," and they were in unison, rowing like fury. In the middle were the haemophilia directors and it was pretty chaotic. They didn't quite know -- and at the far end were the civil servants, and every time we got near land, they kept trying to jump out.

That's how I used to describe it, and that's how I felt it, to be honest. If we had all stayed in the boat and rowed together, I think we might not have had this comment in my first sentence that I was disappointed with the way we handled it; and it didn't get better, as we will see with Hepatitis C.

Q. Can we turn to the next page, please? The letter you mention in paragraph 2.01 is the letter of 15 February 1984 to Dr Bell, at which we looked a moment or two ago, and that's the one about research, and we had the pure research/applied research discussion on that.

You then go on to say what happened when you visited Professor Weiss' laboratory at the Chester Beatty Institute in London.

Professor Cash, I would like to put to you that your date might be wrong and we are going to, I hope, try to establish, as far as we can, what happened around these events. First of all, I would like to take you to the two references you mention in 2.02 and we are going to look at them in reverse order. [SNB0074920].

THE CHAIRMAN: Ms Dunlop, do you want to consider a break?

MS DUNLOP: Yes.

THE CHAIRMAN: Or would you rather have this confrontation first?

MS DUNLOP: I think we can finish this and then perhaps have a break if that's acceptable, sir.

THE CHAIRMAN: It is.

MS DUNLOP: Right.

These are two letters of Dr Perry's and this is him writing to Luc Montagnier in February 1985. It is an interesting letter from our point of view. I really can't resist saying that it mentions Dr Cuthbertson and anticipates there is going to be liaison between Luc Montagnier and Dr Cuthbertson, and almost the first letter that takes that plan forward is from Dr Cuthbertson and it's all in French.

I think I'm just manifesting the usual British person's incredulity that we can actually do that.

Dr Cuthbertson corresponded with Luc Montagnier pretty promptly after this to try and take forward the plan, and we can see from the letter that actually there are two different topics. Firstly, a hope that the Institut Pasteur will be able to assist with studies into the effectiveness of viral inactivation. So in early 1985 it's important to PFC to be able to demonstrate that their heat treatment is working and they are trying to enlist the help of Luc Montagnier with that project.

A. That's correct.

Q. And we know -- and you alluded to this last time actually -- that there were some difficulties about attempts to try to get the virus and questions of containment and so on, stocks of virus are referred to. We don't need to go into that just now. What's also interesting is that last line:

"The SNBTS is preparing to evaluate LAV/HTLV-III test kits with a view to routine introduction of such a screening test in the future. It may be to our mutual advantage if we could extend the collaboration with your centre to include an evaluation of the kit you have developed."

We are seeking some further information on this at the moment, sir, we are not very clear actually what happened to this initiative.

A. It didn't.

Q. I think by a process of elimination it is apparent --

A. If you read the Gallo-Montagnier -- I would hesitate to say this but the lawyers moved in. There was great sensitivity, understandably. But it didn't.

Q. Well, to note from this letter that in February 1985 contact has been made with Paris on these two points, and then the other letter, which is [SNB0075427], this is Dr Perry writing to Professor Weiss, and he is talking about the same thing: heat treatment studies.

So HTLV-III inactivation studies in blood products. And he is alluding to discussions some time ago regarding a collaborative study.

Dr Perry mentions that:

"Technical and Health and Safety Executive problems at our end delayed the initiation of such studies."

And you have alluded to that before.

A. Yes.

Q. So there has been a bit of a hiatus, not least also there is mention of the fact that Professor Weiss moved his laboratory, but there is a wish to reactivate the planned cooperation. Dr Perry says:

"Our ability to demonstrate its safety ..."

That is the heat-treated product:

"... is of paramount importance."

So this is as at November 1985.

A. Yes.

Q. And reference to Dr Cuthbertson's work with model viruses, which we can remember from a couple of weeks ago. Dr Perry is saying:

"In the current climate, we need unequivocal evidence of inactivation of the real things in order to avoid an imminent crisis of confidence."

Then on to the next page just to complete that letter, please.

So those two letters are your two references and they are in 1985. It seemed to me, Professor Cash, that you had initiated the contact with Professor Weiss because you talked to him after the first meeting of the EAGA, the Expert Advisory Group to AIDS.

The first meeting of the EAGA was 29 January 1985 and you seem to have got into conversation with Robin Weiss after that and made perhaps the suggestion of some form of collaboration. If we look at [SNB00074904]. Here we are. This is you, to use your words, getting the show on the road.

A. Could I comment.

Q. Yes, by all means.

A. It is very interesting, very helpful. I have the vivid memory, before all these letters from Bob and Bruce -- for instance, if you go to Luc Montagnier -- and I regret to say that one of the sadnesses of this Inquiry for me is that a whole lot of my files were destroyed in 1992 and they are very early 1980s, the first half of the 1980s, and so I haven't got paper back-up. But I have this clear memory of being encouraged by a Dr Habibi, whom I knew very well in Paris, to go and see Luc Montagnier. And I went over there. While I was seeing Habibi and other people in Paris -- I think Jean-Pierre Allain I also saw. Someone

I know you have been in contact with.

So I went to see Luc Montagnier with a view -- because the virus inactivation in vitro concept was one that Mr Watt, originally, as I recall, discussed with an extraordinary FDA guy called David Aronson in the 1980s -- 1981. And the moment I was aware of what Montagnier was doing in terms of HIV, I went over to Paris to see him and my other colleagues as well, and I remember two things about that: that he was very courteous, he was very cool but he only spoke French, and I was ashamed, to be honest, of my lack of fast French and so on.

But he was very charismatic and I came away saying, "I don't think we are going to do this," and passed this information on to Bob Perry. I didn't tend to interact directly with Bruce.

I have another memory of going down to the Chester Beatty Institute and meeting Robin Weiss. Now, he is coming up and he may or may not remember this. My vivid memory is I sat down at the bottom waiting to be called up to see him. His secretary came down and it turned out -- and I have never seen anything like it in all my life before -- it was a man was his secretary, in pink trousers and a pink shirt. And I remember being quite taken aback. At that time HIV and so on and so forth.

And he courteously took me up to see Robin, and I'm absolutely sure in my head that my first contact -- and I'm sorry I have no documents -- with Robin was not in Hannibal House, it was way before then. And it was about HIV, as I described, about getting some virus, please, to allow us to do the testing.

Q. Right. I think --

A. So, I'm sorry -- I mean, I know where you are going and I can see the logic of it and I can only give you my memories, I am afraid. I have no paperwork.

Q. Yes, indeed, professor. If we just look back at your statement, I think that if one looks again at your comments, particularly in 2.03, about your having visited Professor Weiss and discovered that there was the RIA and then you put the letter from Dr Gunson, which we have already looked at, of 3 July 1984, after your visit and the discovery of the RIA, what I'm saying to you is that that can't be right because we have already looked, no doubt slightly tediously, at a whole load of documents that show that the RIA wasn't available until some point in July 1984. So you can't have been at Professor Weiss's laboratory noticing the RIA early in 1984 --

A. I'm not suggesting I did notice, but in the course of conversation it became clear to me -- and I mean,

I assume it became clear to me -- that they were pursuing a diagnostic test and it was the RIA. That led me to buzz Harold Gunson.

Q. Yes. I think it's really just that the --

A. I don't wish to split hairs.

Q. It's possibly slightly more than that because the implication of your paragraph 2.03 is about you or Scotland being cut out, and I wonder if that's really right because we know that Dr Gunson arranged this meeting -- or the meeting was arranged -- at the end of June 1984, and that Dr McClelland was invited, couldn't make it for logistical reasons, and that the RIA emerged very shortly after that.

So it doesn't look as though there was some sort of process going on from which you were excluded.

A. No, I wasn't -- you see, the fact that Brian McClelland was there, whether he was there because of a train or whatever, wasn't important to me. The fact that I wanted was that there would be a centre, and it would have to be the West of Scotland for the reasons that you now know, that would in fact take part in the assessment of the RIA and the ELISAs, so we could get a good comparison.

That was my understanding of my contact with Harold, who was an old friend, that we wanted a Scottish

regional centre and that it would be the West of Scotland.

When we came to HCV, this was all very straightforward. It was well rehearsed and we were into it. I was concerned that, for whatever reason -- and it may be that my lack of enthusiasm for an RIA approach impacted on Harold a little, because I confess that I wasn't greatly enthusiastic with him when we chatted, and that -- he may say, "We will keep the Scots out". In fact, the West of Scotland was one of the crack RIA -- Edinburgh didn't do it in terms of HBsAg. So we had a lot of expertise on RIA in Scotland.

Q. I think it's just about timing, Professor Cash, it's not a big point.

A. No.

Q. But I think that both the availability of the Chester Beatty RIA in the middle of July 1984, and indeed the availability of the United States commercial ELISAs, may have been slightly later than these paragraphs in your statement might suggest. That is relevant when one is trying to look back and discover what was possible when. I think that's really my only point.

A. When did they publish the RIA Chester Beatty assay? Was it 1,000 donors they did?

Q. The 1,000 donors were looked at over the summer of 1984 and the paper was published on 1 September.
A. Are you telling me they didn't have the assay until July?
Q. If the contemporaneous documents are right, yes, it looked as though the assay --
A. Certainly I'm suggesting they were talking about it --
Q. No doubt it was obviously what they were trying to do.
A. Robin Weiss was a very open kind of a chap.
Q. I think that would be a good moment at which to break.
(11.16 am)
(Short break)
(11.35 am)
THE CHAIRMAN: Yes, Ms Dunlop?
MS DUNLOP: Right. Thank you, sir.
Professor Cash, just before we stopped we were at about paragraph 2.03 in your statement, and I think I was trying to persuade you that if you were disappointed in the summer of 1984 that Dr Gunson hadn't mentioned assessment of available US ELISAs in his letter of 3 July, that was possibly a bit premature because we know that Abbott only picked up their isolate, for example, in the jet, in June 1984.
So really, in this whole passage I'm suggesting to you that the timings of events are perhaps slightly

later. And that Dr Gunson may not have mentioned assessing the US ELISAs because they simply weren't available as at 3 July 1984. Is that possible?

A. It's possible but I would just add a rejoinder -- I can't let it go entirely -- that there was an enormous amount of cross-Atlantic buzzing going on between myself and others and professional blood bankers, particularly people like Tom Zuckerman and so on, and Bill Bayer in Kansas City. And certainly I was aware at that time that there was a lot of activity going on in the US diagnostic industry, and --

Q. Yes.

A. -- I take the point you make but the notion that we should be in there, picking up technology and comparing it, was, I still think, important and relevant.

Q. Yes, absolutely, and you go on to say in 2.04 that you saw Dr Gunson's letter because Dr McClelland sent a copy of the copy he had received to you, and you say that:

"Both Dr Gunson's passion for an RIA and his passion ..."

I think you mean that the kits that had been manufactured in BPL had been challenged. Certainly from the letter of 3 July, it looked as though -- perhaps it was a little short of passion. I mean, the option of manufacture at BPL is mentioned certainly but is said to

be one of a number of options. But you obviously recall there was a keenness on RIA, and we can certainly see that from the documents. What I'm suggesting is the other half of that, the desire to have the kits manufactured at BPL -- certainly there is mention of Dr Lane being keen on that but Dr Gunson seems to have recognised that there were other possibilities.

A. Oh, yes, he is a wise old owl, but Harold sat on the board of the CBLA, as did Richard Lane, and -- I'm sorry I can't quickly but I can dig it out. There is a minute of the CBLA meeting in which the board, which included Duncan Thomas and so on, supported the notion and the chairman talked about it, that it would be much cheaper to the NHS, and I have already commented about that.

Q. Yes.

A. So in other words, the board of CBLA were just as keen, in my view, as anybody else. I talked about passion because, as I think I have subsequently said in my statement, I use that word with Harold for the reasons -- but I was far from convinced that he had behind him a vast army of passionate NBTS directors, certainly I know one who wasn't.

Q. Well, I think --

A. So --

Q. I hope we will go on to see he didn't communicate his

passion to the DHSS or if he did, they didn't share it. We know from the narrative of events that the ultimate British test wasn't an RIA and it wasn't manufactured at BPL. So events really tell their own story perhaps.

You go on to say in 2.05 that by late December 1984 there was deep concern among the SNBTS directors. I think we need to look at the minutes of the meeting of 11 December, the directors' meeting. That's [SGF0010137].

I alluded to this earlier when I was trying to paraphrase it but if we look at the real thing, we can see that this is the directors' meeting and we can look particularly at page 3, please. We see the two McClellands. There is a whole heading "AIDS" and Dr McClelland had found the 27 November 1984 meeting of the working group disappointing. Then the British cell line, unanimous agreement to test all donors once an antibody test was available:

"The care of antibody positive donors was acknowledged to be a very difficult problem."

Then you go on to say that you are going to make further representation to SHHD that there should be a more effective coordinated UK approach to transfusion and AIDS, and the directors are noting with regret that a second meeting of the working group had not been

arranged. Then if we could read over, please. There appeared to be no evidence of co-ordination of the many splinter groups which existed.

Can we go back to Professor Cash's statement, please. So these minutes recording a number of concerns as at December 1984, as you say. You then tell us that "we", which I think is the directors and yourself:

"... had evidence that the FDA was now well advanced in its assessment of HIV donation screening kits, which was later published."

That reference, which we are not going to go to, is a letter in the New England Journal of Medicine on 2 July 1985. It's a write-up by Petricciani and others of the process of assessment.

I did want to look at an earlier intimation of what was happening towards the end of 1984, which is in Nature again. [PEN0170658].

We can see this is Nature for 13 December 1984. The magazine talks of the crash effort by the public health service to develop a blood test for AIDS, and that it had run into difficulties:

"Pilot trial indicates that the test kits being developed by five different contractors show wide variations in their sensitivity and selectivity and may be subject to significant false positive and false

negative rates."

Reading down:

"In the recent pilot trial, the five companies were each supplied with approximately 3,000 samples from plasma donations and 3,000 from whole blood donations."

And the writer goes on to say in the next paragraph that:

"Any interpretation of the findings is complicated by the fact that the five companies each received a different set of 6,000 samples."

So that can't have made comparison between them very easy.

A. Not very good design.

Q. Well, part of the explanation seems to be given by Lowell Harmison of the Office of the Assistant Secretary for Health:

"Namely, one object of the trial was to assess the prevalence of AIDS in the population."

So it may have been difficult to do both in the one study: measure the effectiveness of test kits at the same time as measuring the prevalence of AIDS in the population. Possibly a little ambitious.

THE CHAIRMAN: It also seems difficult to form any general conclusions from five particular examples which lack coherence.

A. And there is huge population differences in the USA of this condition, yes, indeed.

MS DUNLOP: I think he is a doctor, isn't it he, Dr Petricciani?

A. Yes, I knew John quite well. A big man.

Q. He is saying:

"Some companies have more work to do than others in refining their tests. The primary purpose of the trial was to provide an early look at the performance of the test kits."

So it certainly doesn't read as though everything was running particularly smoothly with the development of these kits.

Can we just look at the rest at the top of the page, please, the rest of the piece? A comment on the implications of a false positive rate of about 10 per cent and a Dr Peter Page -- did you know him?

A. No.

Q. No. He is concerned that the tests may be rushed to market before this false positive problem is corrected. He said:

"We are being rushed so much by Margaret Heckler that we don't have time to resolve them."

That is the problem. That particular concern that the tests might be rushed to market before the false

positive problem had been corrected, that was one you shared in the spring of 1985?

A. Yes, we were just sitting on the sidelines, all of us, including these guys, as the Montagnier versus Gallo played out. And it was a big race and Margaret Heckler, Secretary -- these are politicians and it was about securing the market for American manufacturers. That was what the --

Q. And of course, as we saw earlier, on 23 April she had said a test within six months.

A. Yes.

Q. Right.

A. You know -- but we were sitting there like stookies on a wall. The British had not got their act together and had not actually got their talons into the game.

Q. Yes.

THE CHAIRMAN: Do you know who Stephen Budiansky was? Was he medical?

A. No, sir, I don't.

THE CHAIRMAN: We see him from time to time.

A. I don't. This article is referred to in the major publication, I think in the Lancet, of the PHLS, Mortimer/Tedder study, as anonymous. The reference is anonymous. And I remember it vividly. When I went to it, there is a name.

MS DUNLOP: Yes.
A. So it's a mystery, sir, I'm sorry.
Q. Right.
A. I knew Lew Barker, the director of the American Cross, very well and the fractionator.
THE CHAIRMAN: Professor James is suggesting Budiansky may have been a Nature staff writer.
A. Indeed, that is a very good point.
THE CHAIRMAN: In which case, to the outside world he would be anonymous.
MS DUNLOP: To go back to the statement, [PEN0171038] at 1040, you mention again Dr Covell, and you say:

"He was selected to liaise with DHSS and he had no knowledge of blood transfusion matters."

Professor Cash, from the documentation, particularly the SHHD documentation around this time, there is a very clear, continuing presence of Drs Bell, McIntyre and Scott in all of this debate.
A. Very much.
Q. So I don't think one could suggest that it was all being left to Dr Covell, who didn't know what he was doing as far as transfusion was concerned, could one?
A. No, but I wouldn't wish to criticise but, as you went up -- this is normal. I'm sure you have interviewed some people here from the SNBTS, who would have said to

you on reflection that Professor Cash knows nothing at all about that.

As you went higher up into the decision-making areas in the department, these guys, like Graham Scott, were very dependent on good briefing from people on the ground. So I wouldn't wish, please, to imply that Dr Covell was a disaster, a problem or anything like that; I'm simply saying that the continuity -- at that time, I and my colleagues were very frustrated and actually quite angry at what was going on, and he felt that the Scots were not fielding their best team in terms of knowledge and background, for very probably very important staffing problems that -- I mean, Iain McDonald used to go on continually to me, and I understood this, that the medical staffing at the Scottish Office in that period -- it probably applies to today, I don't know -- was very difficult.

Q. Well, you've --

A. I wouldn't wish to criticise --

Q. You have proffered yourself an explanation for why Dr Covell might have replaced Dr Bell at the working group meeting on 27 November 1984, a meeting which, as we have seen, everybody thought was disappointing anyway, but there isn't really, as far as we can see, much evidence of Dr Covell playing a particularly

prominent role, although I think he did go to EAGA.

A. Yes, he was later to get the poisoned chalice, that I think were the alternative testing sites, which no doubt we will come to.

Q. Right. You go on to mention Dr McClelland's letter to Wellcome, which we will look at. That's [SNB0059501]. You are mentioning in this context the possibility of actions being dictated by a need to let Wellcome catch up.

A. Yes.

Q. And that would be to catch up with the American manufacturers. If we look at this letter, we can see that it's dated 8 January 1985 and Dr McClelland is writing to a Mr Madden of Wellcome, during your absence. He is really just wanting news, I think. That's how the letter reads. He says:

"It has been a matter of great concern to us ..."

He says in the first paragraph:

"... since almost a year ago. There has been optimism that some form of antibody screening test would be available."

But it doesn't sound as though there is going to be a test ready for implementation any time soon. He says he is concerned at the apparent lack of progress and he is looking for reassurance from Wellcome. The

possibility of even getting a limited supply of materials for HTLV-III testing.

Quite a lot of the material from around the end of 1984 and the beginning of 1985 seems to suggest that Wellcome were thought to be developing the test for the British Blood Transfusion Services. Is that right?

A. Yes, absolutely no question.

Q. Yes. So really have that perception and then the emergence of an idea that all available tests will be subject to an evaluation process, and that that will have to include the Wellcome test.

So perhaps a change from a sense that Wellcome were just going to move into the British testing arena to a position where they would have to compete with the American commercial tests to see which was or were the most suitable, before the British Blood Transfusion Services would decide what test to use. Is that a reasonable summary of what seems to have happened?

A. Yes, I think there was -- there was certainly within the Department of Health in London, and probably in DTI and this British Technology Group -- that there was a major need to develop a British kit. There was, in my view, a total false assumption -- and it's my view -- that that would automatically be imposed upon the UK Blood Transfusion Services.

Two reasons why I would say that: one, is that the politicians were already -- I think there is a reference to it -- being hounded by the pharmaceutical industry and the kit people about the development of a British test, and they would be excluded from being considered. Secondly, if you talk to people like Ruthven Mitchell and Brian, but particularly Ruthven Mitchell and his team, they in Scotland desperately wanted an option, (a) in terms of bargaining financially but, (b) in terms of the technology.

Now, in this area Wellcome ultimately succeeded, at which we were all absolutely delighted, but I notice that two publications don't appear -- and they may be in the court book -- in which two Scottish centres, Edinburgh and Glasgow, are very critical of the Wellcome test in that fist six months, in terms of its specificity. They cracked the technical problem, I'm delighted to say, and Wellcome were very grateful.

Q. Yes.

A. But the notion of a single source for such an important exercise as this is something that we would be instinctively concerned about. So there was a sense that if that existed down in London, we were not on the same planet, as it were.

Q. I think we should just look at the end of this letter.

He says he can't over emphasise the urgency of the situation. I think perhaps the only point one might pick up from your statement, professor, is your suggestion that the extent of the catch-up for Wellcome as at the beginning of January seemed substantial. It didn't really look from the Nature report as though the American tests were in a particularly good place either. It looks as though everybody was having difficulties.

A. Well, again there is a lot of transatlantic communication, but within weeks American kits were being shipped over to Richard Tedder and Philip Mortimer for their so-called phase 1 study, the DHSS study.

I think one reads that other document, the big document you referred to, that the Gallo team and Abbott and so on, were struggling, it would appear. The fact of the matter is that when Tedder and Mortimer, who actually were DHSS procurement people, went out to the market and said, "Who is going to offer us some kits to test?" There were at least five immediately available for them to do it. We were sort of aware of that.

Q. We will come on to look at some information about actual availability but can we return to the statement, please? We see you making the point in 2.06 that the key information urgently required was specificity; in other words, you are talking about the avoidance of false

positives?

A. Yes.

Q. So a positive is a true positive, in other words?

A. Exactly, that's not a problem.

Q. You say that you also believed more technical effort was needed in the area of confirmatory testing because of this vexed problem of what to do with donors who had positive results that might be false positives.

A. That's right.

Q. That reference, [PEN0170649], we can just glance at that, I think. That's the article from Transfusion in 1992, with which you have provided us, and I referred to it earlier because it sets out some of the obligations that are owed to donors.

We can see the third paragraph:

"Blood bankers have inescapable responsibilities to these donors. Donors have to understand the criteria for their candidacy but also the reasons for deferral when those criteria are not met."

That certainly seems, Professor Cash, to be quite a useful discussion of the sorts of dilemmas that occur.

A. Yes.

Q. If we look at the second page, we see a sentence near the top of the left-hand column:

"There is scant enlightenment, let alone

consolation, for the donor deferred with a 'false-positive' result and given the explanation that the predictive value of the screening tests for antibodies to HIV ... is only 10 to 30 per cent where the sero prevalence of the antibodies is 0.04 per cent."

I think what that's saying is that it's not particularly helpful if the donor is told that there is a positive result, to be told, "Well, statistically it's not that likely to be true".

A. Yes, absolutely. I mean, if -- I mean, people like Jack Gillon would confirm that. If you had actually done it and sat with people and tried to counsel them, it isn't very pleasant.

Q. No, I think we can appreciate the point you are making. Yes, at the bottom of the left-hand column:

"The indications for new screening tests must be well established and the tests introduced must be of high specificity. Manufacturers must be encouraged to develop confirmatory tests that can be licenced, if not at the same time as the corresponding screening test, then certainly sooner than has been our experience with both anti-HIV-I and anti-HCV testing."

Can we go back to the statement, please? In 2.07 and 2.08 you are talking about the position in Scotland in January 1985, and we need to look at some

correspondence that evidences that. Can we look firstly, please -- because we will do this chronologically -- at [SGH0027301]. This is an SHHD minute or memo. It's from Dr Bell. Can we just quickly scroll down and back up again so we can see. Yes, A E Bell, 21 January 1985. Dr Bell is writing to Mr Murray, who we know to have been a non-medical civil servant, and copying it to Dr McIntyre, who is the level above Dr Bell.

A. That's correct.

Q. It's about screening and we can see that Dr Alison Smithies has sent to SHHD -- actually I think it's a Mr Williams on behalf of Dr Smithies -- the submission which DHSS have prepared and sent to English ministers. So the submission has been sent, no doubt for assistance, to SHHD, to let them see the thinking in the Department of Health and Social Security. Dr Bell records that:

"It's helpful and convenient to see that submission."

There is a planned meeting with Dr Smithies and Mr Williams on 4 February. Dr Bell goes on to say that:

"The regional transfusion centre at Law is testing the Abbott test, which, as indicated, uses the less convenient ELISA technique and requires certain

dedicated equipment, supplied from the manufacturers at appropriate cost, of course. I would hope that the SNBTS would wish to use the new UK test and that this test would be made equally available north of the border."

That's obviously the Wellcome test he is meaning.

A. Yes, I think he is meaning RIA as well.

Q. Yes, indeed.

A. I think.

Q. At that point he perhaps is under the impression that the Wellcome test will be an RIA?

A. I think, I don't know.

Q. He goes on to talk about cost. If we go right down to the bottom, we can see that Mr Murray has added some thoughts of his own on cost. He is writing to Mr Davies, who is immediately above him.

A. Yes.

Q. Mr Davies is saying that:

"We must alert the finance division."

So that's the position as at the 21st. Then can we look back at [SNB0057304]? We looked at this letter in quite some detail earlier but just to place it in this mini chronology. This is you writing to Dr Bell on 24 January?

A. Yes.

Q. Recording various concerns, principally in relation to the disappointing meeting of the working group in London in November and the lack of, I suppose, clear direction at the top of NBTS; yes?
A. Top of the Department of Health, rather than NBTS.
Q. Yes, and concerned about delays on the screening front and mentioning the contact that is already taking place with the American companies.
Then we go to [SNB0059715]. Not quite chronological actually. Slightly reverse. This is Dr Mitchell --
A. Yes.
Q. -- writing to you on 21 January, so the same day as the Dr Bell memo. He thinks the two of you are in a telepathic relationship.
A. We were.
Q. Right. Dr Mitchell has had a visit from Abbott, and so had you?
A. Hm-mm.
Q. Yes?
A. Yes.
Q. And this is all with a view to starting some evaluation of the Abbott system.
A. I think it was more than that. The visits were, "Can you get your order book out and tell us when you are starting --"

Q. How soon would you like it?
A. This is the market boys.
Q. Yes. Then mentioning MMWR. And don't worry, we will go on to look at that. Someone has written, presumably helpfully for you -- that's not your writing, I don't think?
A. No.
Q. "In basket today from PF."

So Peter Foster has had the MMWR and he has put it back in the basket for you.

A. That is Elizabeth Porterfield, it is, the writing, I think.
Q. Okay. Some admin really to do with conducting work on the Abbott kit. Then can we look at the following page, please? Wondering what to do as far as ethical approval is concerned:

"Abbott are hoping to have the study completed by the end of March."

You have obviously actually spoken to Dr Mitchell on the 21st.

Then your reply, [SNB0059713]. So you wrote back to him, thanking him for his letter. You also thank him for contributions to discussions with Dr Brookes?
A. Yes, Ewa, Bill Whitrow and Brian McClelland.
Q. He was a regional director?

A. Inverness.
Q. Inverness, right, and Brian McClelland last week.
A. Yes.
Q. TPH?
A. Trinity Park House, which was the CSA headquarters.
Q. Yes, we know it well, or we knew it well. Thank you.
A. Yes.
Q. And you are encouraging Dr Mitchell to proceed with this evaluation.
A. As I recall, the four of us met because -- I haven't quite confirmed this -- because there was a BTS subcommittee meeting. There was some reason why we were in Trinity Park. It was not a place we normally congregated and we were there together, and Ruthven and I were the same, and we sat down and talked through the January position and decided, "Look, we have just got to do it ourselves". So -- and this was the follow-up, yes.
Q. Right. So in fact you are not restricting the work that's to be done in the west to the Abbott kit; it's a more general intention --
A. No, the idea was to go out and -- yes. We knew from the Abbott Guys -- you see, we were close to Abbott, or Ruthven was, because he used, in preference to the BPL one, the Abbott RIA for Hepatitis B surface antigen.

Q. I see.
A. So they were very familiar with the scientists and so on, but we knew that there were others; within weeks that was very evident, yes.
Q. Yes. Then you are giving some practical steps and then on to the next page, please. You are emphasising:
"We must not be stampeded by any of these companies, nor enter into this exercise seeking quick scientific publications."
So you are saying, "We mustn't rush it"?
A. Oh no, I think -- I know where you are going there.
I'm saying, "We have to rush it but we really mustn't be messed about by these people." There was immense competition going on. "We do not think you should do that one, do ours instead". And Abbott, I think in particular, at that time wanted an exclusive access to -- I don't think it's appreciated that the West of Scotland kit assessment team was a world beater. I mean, I have to say this. I take no personal credit. It was a world beater, Archie Barr and that team there. And for the companies -- and this became very evident when we got Hepatitis C -- for the company to get access to it with the possibility of publication was very attractive, and all sorts of efforts were made to get exclusive access to it.

Q. Right.
A. So that was around that. But, no, we weren't -- we were in a hurry to get an answer but we needed to get an answer that was independent, so we could compare other people as well.
Q. Okay. Let's look at what MMWR were saying. That's [SNB0049195]. We need to look at the foot of page 2 in fact. Screening blood and plasma. This is their recommendations, CDC recommends:

"All blood or plasma should be tested for HTLV-III antibody by ELISA. When the ELISA is used to screen populations in whom the prevalence of HTLV-III infection is low, the proportion of positive results that are falsely positive will be high."

At the moment I'm not completely sure that I understand why that is necessarily so, but we are going to ask Professor Weiss about that.

A. Thank you.
Q. Right. Then they go on to recommend that:

"The ELISA should be repeated on all seropositive specimens before the donor is notified."

Then it mentions other testing. So there seems to be almost an acceptance of an inherent false positivity problem, at least with the American tests.

A. Yes.

Q. It looks as though that's what CDC were saying.
A. Yes, indeed. I would almost say it's a problem for every test that we have introduced. It is a principle that's a problem and is a matter that requires very sensitive and careful management. Yes, absolutely right.
Q. And then, if we look at your own letter, yours and others, in the Lancet from March 1985, the same point about the false positive problem is made. It's [LIT0010374].

This is actually a longer letter, this first letter, which has some relevance too. It's a letter from the United States about screening.

Then can we look at the next page:

"Considerable effort is being directed at the development of a screening test ..."

We can see that this is actually from California, a number of authors from California.

A. I knew Paul Holland quite well.
Q. Right. It talks about a particular analysis that has been done there with the Gallo cell line, and if we can scroll a little bit down, we can see 9.2 per cent had -- I think that's positive/negative ratios. We are going to ask Professor Weiss to explain a bit of this to us as well but I suppose satisfied the definition of

a positive result by ELISA. And these were re-examined by immunofluorescence assay and Western Blot and one was found which contained antibodies to HTLV-III. Perhaps the difficulty for blood services is encapsulated in that paragraph:

"We want to be able to identify all true positive results, without jeopardising the blood supply by unnecessarily deferring blood donors or alarming donors by mentioning a positive test that does not represent true infection."

So this study, suggesting one particular setting for the definition of a positive result, would have resulted in 9.2 per cent of blood units being discarded, but only a single unit would have been discarded if further confirmatory testing had been used.

A. That's correct.

Q. So that's one piece of information, but that's March. So they are saying:

"It's necessary to use the more sensitive ELISA and then use a confirmatory test."

A. But I think I knew from John Petricciani, that we have already mentioned, as this was being published -- and we were getting all very anxious about it -- that 10 per cent -- I should say this is screen positives. This is the first time you do anything, screen

positives. A decision had been made in the UK: if you were screen-positive, even although all the confirmatory tests were negative, you were off the donor panel, you will not be asked back. And lay people found that very difficult to understand: "If I'm okay, why can't I give my blood to people that need it?"

In other countries -- I think in the States; I'm not quite sure -- reinstated people who were screen-positive but confirmatory test-negative. And this is a 10 per center. I was on the phone to John Petricciani soon after this publication and he said:

"Actually, the tests they are now looking at, the kits, we think it's actually down to about 1 per cent."

Q. Right.

A. So it's improving all the time, he said, it's a changing field.

Q. We can see you are one of the signatories of the letter that appears on the right.

A. Yes.

Q. Along with a lot of other transfusion directors.

A. Yes.

Q. And as at March -- I think you sent your letter maybe towards the end of February, but as at that point you were very concerned about false positive results.

A. I might just say that letter was -- I drafted it; it's

mine.
Q. Yes.
A. So I take responsibility for it. It must have drafted in late January -- mid- to late January. The reason I'm aware of that is it took an awful long task to get everybody to sign it. It's like herding a lot of cats: Very, very difficult. The most embarrassing feature of that letter is a missing name and that's Dr Harold Gunson.
Q. All right.
A. And that required me to go down to Manchester to talk to him, to find out why he was not prepared to sign that document, and the answer was he knew in advance that the DHSS were totally opposed to what was being said and so he was of a view he couldn't, therefore, sign it.

But, yes, that's a document saying, "We the transfusion service people, with our expertise in screening donors, need urgently to get on and test these things." And the second thing it was saying is, "We need to actually be thinking now of alternative testing."
Q. Yes, the DHSS -- the second paragraph of your letter, saying that in principle screening is to be introduced but not until test systems have been appropriately evaluated --

A. Oh, yes.
Q. -- I mean, DHSS agreed with that, did they not?
A. Yes. I wouldn't wish to second guess them. The real problem is who was going to evaluate it and how was it to be done, and I can assure you in January whatever it is, here, what was moving is this -- we will come to it later -- in which the transfusion services were excluded from being involved until very late in the game.
Q. Right. Can we go back to the statement, please? We have established, I hope, the position in January, what the concerns were and that you were initiating some evaluations in Scotland, principally, or perhaps exclusively, in the West. But then you say that you were invited to discuss the situation with Dr McIntyre and that he made it clear that SHHD was strongly opposed to the prospect of SNBTS undertaking its own kit evaluation:

"SHHD had given an assurance to DHSS that they were content with the proposition that kit evaluations would be managed by DHSS and the commencement of routine donation testing in Scotland would be determined by ministers on the advice of DHSS and this date would apply across the UK."

Then you say:

"As I recall, I thereafter consulted with

Dr Mitchell and Dr McClelland and we agreed that, in view of the hostile reaction of SHHD, this SNBTS initiative should be stood down."

We did put that to Dr McIntyre, and his response, which has come by way of an email, will go into court book, but for the moment I think I just need to say to you, Professor Cash, that Dr McIntyre takes issue with paragraph 2.08. He doesn't remember making the statement you say he made. He is not sure if the hostile reaction is meant to be him but he would deny that he ever spoke to you in that manner. He says that SHHD treated you and your colleagues in a professional manner and did all that they could to help.

So that's his position.

A. Yes. Would you like me to respond?

Q. You may, of course, comment on that response.

A. Yes. When I say "hostile", the conversations, as I recall -- no records, sadly -- got a little bit heated when Archie -- and, you know, he had a job to do -- made it very clear that the views he was expressing would be transmitted to the CSA and, as I think I have said in my document, in terms of funding, all we needed was overtime payments, and the authorisation of these overtime payments would be in the hands of the Treasurer's team at the CSA and so on, and it got

a little bit -- Archie wasn't hostile in the sense of being rude -- he was always very courteous -- but it got a bit tricky when it was quite clear that ultimately the department could control what we were doing by the funding arrangements through the CSA. That's really all. That's actually what happened.

Q. Right.

A. I wouldn't wish on record to put that there was any personal animosity; there wasn't. He had a job to do and he was telling me decisions that had been made, above him no doubt -- and I respected that -- and as a consequence of which, we pulled out.

Q. Right. You suggest that this may have triggered your letter of 12 February. We looked at that before. Can we just glance at that again? That's [SNB0132233].

You remember that was the one that had the three-point plan and then the suggestion of a statement with three elements to it, the "madness afoot" letter, in fact.

If we look at the second page, please, you were actually favouring, from your suggestion in relation to the statement, a national kit evaluation programme?

A. Yes. I had favoured that all along but by January 1985 we reckoned it wasn't going to happen, so we decided we had a duty of care ourselves to get on and have a look.

Q. I suppose you could have done both.
A. Oh, yes.
Q. And then maybe --
A. What was not happening was the national programme was not moving.
Q. Well, at least you weren't aware of what was happening inside DHSS in relation to that.
A. No, we knew something was going on but we were not privy to that and, as far as I know, neither at that point were my Scottish SHHD colleagues.
Q. Right, but Dr McIntyre, I suppose, might have been pointing out that there might be duplication of effort, if you were setting up this evaluation in the West at the same time as the same kit was being evaluated under the aegis of the DHSS in London. Would that not be a legitimate point for him to make?
A. It would but I would argue that the effort was ours and if we felt it was worthwhile in terms of discharging our duty of care, then the responsibility for worrying about duplication, I would argue, would have been ours and so we would need to consider this. But the amount of money involved, because we were going to get all the kits free, was very small indeed -- of the investigation that we had thought of at the time.
Q. Yes. Can we look, in connection with the question of

what was available, at [DHF0025475]? Yet another DHSS memo. It's a note about the introduction of testing:

"All manufacturers have been told that there is going to be an evaluation. Five manufacturers have indicated their willingness to cooperate."

There is going to be an initial evaluation, the protocol for which will be agreed by an ad hoc panel of virologists, and then a field evaluation, and that's going to be conducted by those representing the Blood Transfusion Service and a statistician. The kits -- and this is 5:

"The kits which have satisfied the evaluation will be listed by the DHSS."

So in a context where there is no legal power to prevent the marketing, there is still going to be listing of kits, which will no doubt be information that will influence purchasers in their choice of particular kits.

A. We didn't have a licensing --

Q. No. Unlike America?

A. Absolutely.

Q. Yes:

"Regional transfusion directors have pronounced their resolve to commence testing in each centre at the same time."

Again, was that something that you supported, the idea that testing should start at the same point everywhere in Britain?

A. Yes, I did, and we may come on to this later.

Q. Yes. Can we just move through this, please?

Then I think, paragraph 7, we can see actually that's Abbott Laboratories. There is no great mystery about this because Abbott were the first firm to have been given FDA approval:

"Arrangements are being made for their tests to be put to evaluate at PHLS Colindale. There are presently no available kits from ..."

I think that's very likely to be "Abbott":

"... in this country. They are importing a small quantity during this week."

And someone has:

"... agreed with the sales representative to look at the diagnostic kits and the apparatus in a preliminary way."

So it does look, Professor Cash, as though there was a problem in actually getting hold of Abbott kits.

A. Yes, that may be so. I can only tell you that in the discussions I had with Ruthven Mitchell and the team, that led to the aborted Scottish evaluations, I honestly wasn't aware that that was a major problem. That may

well be because Abbott were now well advanced in discussions with Ruthven Mitchell and were ready to move in very quickly because they had worked closely with them, and it may well mean also that we had not yet contacted other companies. I have just no information on that, I am afraid.

Yes, I have seen this. I have seen Robin Weiss's comments about the -- and, to be honest, it has been news to me in 2011.

Q. Right. As you say, there are a number of different sources, all pointing to Abbott's difficulty in making enough kits, and we will look at some of these. But, of course, you can't evaluate a kit if you haven't got it.

A. Absolutely right.

Q. Yes.

A. That's 11 March.

Q. Yes.

A. It's my understanding that the Mortimer/Tedder evaluation of those kits began in March.

Q. Yes. We will hear from Professor Tedder tomorrow but I don't think he was involved in the evaluation, we will come on to that. There may have been an initial expectation that he would be and then realisation that that wouldn't be suitable. But let's come to that in a moment.

Just to pick up, you suggested we should be contacting Dr Dow and we did contact Dr Dow and here are his responses: [PEN0171680]. Actually just look at his comment, which are somewhat multi-coloured, but it does help.

A. Very helpful.

Q. Yes. It has got the paragraphs from your schedule in blue, the questions in red and his responses in black -- which might not be very helpful for anyone reading this at home. We were interested in this point about were the kits in Scotland at the time of your communications in January. Dr Dow says:

"There was no commercial test available for blood donor evaluation purposes prior to 24 January."

I suspect that that wording is really just because of the way in which the question has been framed, it is not an attempt to suggest that they arrived on 24 January, because he goes on to say:

"I realise that around March/April 1985 an Abbott HTLV-III test system had become available from its use in Ruchill but there were apparent problems with numbers of false positives, poor specificity."

A. Yes.

Q. Actually, while we are in Dr Dow's remarks, we can look down and see that there was a mini evaluation of the

Wellcome test and the Organon test in July 1985. He says:

"Supplies of these test materials were extremely limited."

The Wellcome test won the day in the west. Just to pick up a point you made earlier about the false positive problem, can we go forward to page 4 of this, please? I think this may be the point you were making, Professor Cash, that:

"In the West there were tremendous problems with plate validation failures."

Is that it?

A. Hm-mm.

Q. All right:

"The test kit was less sensitive than the original developmental batch tested in July 1985. The west centre managed to overcome the validation difficulties, together with sensitivity issues. Some specificity problems did arise in attempting to identify very weak positive samples."

I'm not 100 per cent sure what that means. I can imagine that identifying very weak positive samples might be a problem of sensitivity but ... Or it may be in relation to trying to decide if a very weak positive sample is a true positive or not, but there are --

sorry?

A. I think that's right. There was this chronic -- I think Robin Weiss will explain to you, I'm sure, much better than I. There was a problem: where are you going to set your cut-off, you know, in terms of the test, and Robin will explain that, and if you move the cut-off in one direction, "Wow, we have very few donors that are coming up positive," and the real question is were you missing out some that were really -- or do you push the cut-off -- extreme and you are picking up a lot -- so this was a big problem, and a big debate developed among the technical people that left me absolutely out of -- I hadn't a clue what they were talking about.

Q. Sorry, professor, I was just going to say that I think those of us -- you very kindly said in your statement that you thought that we had grasped notions of sensitivity and specificity but there's no doubt it's difficult. I think, however, we have understood that generally sensitivity and specificity are in tension with each other.

A. Yes, absolutely right.

Q. You can have a sensitive test which will pick up a lot of potential positives but then you will get a higher number of false positives, ie poorer specificity?

A. Absolutely. Yes, you are absolutely right, and vice

versa.

Q. One of the interesting things about Dr Dow's response is to look at what he goes on to say on the following page. He gives us for information a table for the number of referrals for confirmatory purposes for the first 176,149 tests on West of Scotland donations with the Wellcome kit, and he says that:

"Of those which were repeatedly reactive ..."

So, if there were 73 that were initially reactive and then 31 that were repeatedly reactive, only six were confirmed HIV positive, once the confirmatory test was done. And he says:

"Data on use of the HIV tests on blood donors during the last 20 years showed that around 99 per cent of repeat reactives are false positives.

"In addition in the first few months of testing, the Abbott HTLV-III test was sporadically used in parallel ... Abbott Diagnostics provided kits free of charge for this evaluation, hoping that the poor specificity found in earlier studies had been resolved. In our hands the Abbott test proved less specific (around 30 repeat reactive samples [all false] were referred after testing for a short period of time) than the Wellcome test and as a result this was not considered suitable as a replacement for the Wellcome test."

Some interesting information about the difficulties in this area.

A. I think Robin Weiss puts his finger on -- I didn't know it until I read his recent --

Q. What do you think is --

A. The source of the antigen --

Q. Yes.

A. The two test --

Q. Yes.

A. -- profoundly different and fascinating. I don't understand the science of this. That seemed to make the problem of specificity -- was very significant indeed.

Q. Yes.

THE CHAIRMAN: I think we have had something about that.

MS DUNLOP: Yes.

A. Yes.

THE CHAIRMAN: We shouldn't perhaps have a diversion into that.

A. No, no.

MS DUNLOP: No, it's very tempting but we will press on.

THE CHAIRMAN: It's very tempting, but could I just have one little thing --

MS DUNLOP: Yes.

THE CHAIRMAN: -- which is causing me concern?

This is all about testing in the autumn of 1985.

Dr Dow -- is it page 4, I think, Ms Dunlop? -- referred to use of tests at Ruchill --

MS DUNLOP: Yes.

THE CHAIRMAN: -- rather earlier than that. Was that infectious diseases testing?

A. Yes, Ruchill was regional West of Scotland -- not blood transfusion: regional virus reference laboratory --

THE CHAIRMAN: Right.

A. -- that -- I don't think Eddie Follett was director of it but he was one of the very senior scientists, and after a period of time I managed to persuade Eddie to second himself out of there to run our national blood transfusion virological reference centre.

THE CHAIRMAN: Yes. But at this early stage, that Dr Dow is relating to, would that be infectious diseases patients being tested?

A. Yes.

MS DUNLOP: Yes.

THE CHAIRMAN: Yes. A different use of the test.

A. Yes. I should make the point he said that they were not commercially available and that may be right, ie you couldn't go and buy them, but they were available for investigation and evaluation, and I suspect that's how Eddie --

MS DUNLOP: Yes -- no.

THE CHAIRMAN: I'm sorry if it's is a diversion but it is a contrast within the document I was --

MS DUNLOP: Yes. I have found a Department of Health document, sir, which comes from Abbott, writing to London and saying, "We have already made contact with three laboratories in Britain and one of them is Ruchill." So they had obviously approached Ruchill. I think it's very difficult to work out exactly who was doing what in this confused period but certainly Abbott were very active in making contacts and connections with people who they thought might help in promoting their tests in the United Kingdom.

THE CHAIRMAN: I do have an impression that Glasgow had quite a good relationship with Abbott from time to time in other contexts.

A. No question and -- I mean, Ruthven is very proud they had seen off the Treasury and BPL in the hepatitis surface antigen story and they still remember this with great pleasure, and it was the Abbott kit in that area that was demonstrated as much more sensitive. That was no joke because we were discovering, despite the introduction -- and I'm sure you are familiar with this -- of hepatitis surface antigen testing, Hepatitis B virus was still leaking into the fractionation pools, and there is no doubt one of the

major reason was in that early time the tests were less sensitive than they might have been, and Abbott largely cracked this.

THE CHAIRMAN: I'm sure we will come back to the hepatitis side of it, Professor Cash, but in the meantime I was just wondering about the contrast within this document, and that may explain it.

MS DUNLOP: Yes. Can we go back to the statement, please, [PEN0171038], and now at page 1041? You go on to talk in 2.09 about the evaluation programme. You say:

"Some time after January it emerged DHSS were moving to establish an evaluation programme."

That must have been something of which you were aware because of your discussions about Dr McIntyre?

A. I suspect, yes, with Bert or Archie.

Q. Yes.

You summarise a number of concerns about this programme:

"(a) that UK BTS scientific technical experts, including the DHSS and SHHD consultant advisers in blood transfusion ... "

That's Dr Gunson and yourself respectively, yes?

" ... were excluded from the design of the programme. The design was undertaken by invited virologists with no experience in or responsibility for

large-scale donation screening."

Now, is that not met, Professor Cash, by the fact that phase 1 was, as we have seen, the evaluation of the tests by virologists essentially and then phase 2 was the field study, to be conducted by the Blood Transfusion Service personnel?

A. Yes.

Q. Was that not logical to have that two-part --

A. Totally logical. The worry is -- and it happened -- that the expert academic virologist arrived at the transfusion people and said, "These are the kits that you have to do." Okay? And the problem that emerged is that the design of their experiments -- and I can be very specific -- it's reported in the Expert Advisory Group on AIDS, minuted there.

The problem is that they decided -- and there is good reason for this because of the worry -- I mean, you have no idea. The worry in the diagnostic hospital diagnostic labs in microbiology of the technicians catching AIDS from handling this stuff -- it was UK-wide; it wasn't just -- they decided they would heat-treat the serum in this evaluation before it was evaluated.

The BTS technical staff don't have that luxury. They have to move -- as the donations come in, they have

got to move and deliver an answer yes or no for the release of blood to be used, classically producing platelet concentrates, by the thousand.

What happened was -- and it's in the expert advisory group, and I'm sure I have referenced it here.

Q. You have.

A. -- they heat-treated it and this -- I mean, for those of us half in the game you would say, "Crumbs, that probably will increase the non-specificity, the screen positives," and indeed it did, and the company that took the biggest hit was Abbott.

Q. Right.

A. And so we were very concerned. You know, were there other companies there that had taken a hit, that if in fact they had been tested in the manner that the blood transfusion experts like Archie Barr and his team did, these kits would have been perfectly okay. So there is a logic to a phase 1 and a phase 2. My argument has always been that phase 1 should have been done by blood transfusion virological experts. They had plenty of them.

Q. They didn't test before and after the heat treatment?

A. Well, they eventually did. I mean, if you look in the expert advisory, there was big rumblings from myself, from Zuckerman. In fact I think I have referred to they

had to go and repeat the whole thing and that was another cause for delay and that was the heat treatment thing.

Q. I do very much want to look at that. But I want to clear one point out of the way before we do so and that's your reference to -- and this is in brackets. Before (c) you say:

"A further concern was that one of these virologists was heading the Wellcome Diagnostics HIV ELISA programme and that this was known to all planning team participants."

I don't want to take up time by going through it but, Professor Cash, it does look as though the position was that initially there was an application for funding to scale up the Chester Beatty test, if we call it that --

A. Yes.

Q. -- and also to evaluate commercial tests but that there was an appreciation of possible conflict of interest, and those two aspects were decoupled so that Messrs Tedder and Weiss were not involved in the evaluation programme. Are you in a position to contradict that?

A. I'm in a position to draw to your attention the fact that, although the evaluation did not take place in the

laboratories of Professor Tedder, they did in the PHLS laboratory headed by Philip Mortimer.

What you may not be aware of is that Professor Tedder -- it's published -- was a honorary consultant for the North London Blood Transfusion Service and remarkably, I find, an honorary consultant for the PHLS, and so he was intimately involved in the work that was going on in there. I can only tell you the concerns that were brought to me -- and I had no interest in exploring them -- that if he were involved in developing -- if you were Wellcome or anybody involved in developing this technology or any technology, it was absolutely essential that you got access to samples which were clearly positive by other techniques and so on and samples that were screen positives; you could actually pit your own skills against them.

I can only tell that you a number of the laboratory people that I talked to were very concerned that some of their best samples that they were putting in here were disappearing off to Richard's laboratory for the welfare of the Wellcome Diagnostics. All I'm saying is at that time there was a strong view that Richard -- in fact I can tell you and I'm sure you know I eventually, after three years, persuaded Harold Gunson that people sitting

on the advisory committee for transfusion-transmitted diseases should declare any conflicts of interest, and in 1991 that was finally done and that was the clear evidence to all concerned, that Richard had been involved.

I don't think he was doing anything improper whatsoever. I have no reason to believe that. He had a job to do. There were many academics that I was aware of who were involved in supporting industry and working with them as consultants. I don't have any problem with that whatsoever. But I think in the circumstance, the tense circumstance, of the way this study was established -- it's nothing to do with Richard. Yes, they did not do them in his laboratory, but the notion that he did not know the results and he did not have access to samples I think could readily be challenged, and he in fact is a co-author of the final publication.

I have no problem with that but it was regarded as inappropriate. There are documents now telling us they didn't do the tests in his laboratory because of conflict of interest. It's quite right. So the notion that there was that potential clearly existed in the Department of Health. Certainly it existed outside.

Q. Your position is that you were told that samples that were, what, being put forward to be used in the

evaluation process were also made available to Professor Tedder?

A. Yes. There is nothing improper. If you came up with screen positives -- I mean, the whole issue of confirmatory testing in England and Wales was, frankly, a bag of worms and very difficult. But there is no doubt that the regional centres that were involved in these trials were instructed to ship any positives that came up down to the reference centres, and it's my understanding, and has always been, that one of them was Richard. Richard is a high-class virologist.

Q. We will certainly be able to ask him that when he comes tomorrow.

Can you look, please, at the EAGA minutes and pick up the point that you made a moment ago? [SNB0010432]. You say that:

"The design of the early work was less than satisfactory in terms of UK BTS requirements and some of the work had to be repeated, giving rise to further delays."

I just wanted you to take us, if you would, in the part of the minutes of the EAGA meetings that evidences that, because I was slightly struggling. This is in the EAGA meeting on 30 July 1985, the Expert Advisory Group on AIDS, that is. Can we just move slowly through it to

enable Professor Cash to identify a statement that we need to look at, please? I don't think it's on the first page.

A. Is this July?

Q. Yes. That's your reference.

A. Then it's wrong, I think, I beg your pardon. It would be the May one.

Q. Oh.

A. I beg your pardon.

Q. Leave that with me. We can come back to that.

A. Because this is the one where all the results were finally in and the decisions were made, "Let's roll." I do apologise.

Q. That's fine. We will look out that.

Can we go back to the statement, please? You say at (e) that:

"It was of interest that phase 1 took almost six months and the field evaluation took six weeks."

I suppose one has to bear in mind, firstly, that the field evaluation was not in relation to all five of the kits. The first phase looked at five, or indeed more, I think, more than five, kits, whereas the second phase was looking at a smaller number of kits, which might be one explanation for why it was quicker.

Doing the best we can to work out when kits were

available, if they weren't available to evaluate on 11 March, they obviously did become available when the Abbott kits became available, at some point after that, and the results were ready by the end of July. So it doesn't look as though it was quite six months.

A. It may not be quite six months. The major difference I draw to your attention, if I may, is that whereas the field evaluation was only looking at two kits, it was looking at thousands of donations.

Q. Hm-mm. Thank you.

A. And if you ask how many thousands did the phase 1 do, the answer is 210. It's not thousands. A small number of samples.

Q. Yes.

A. I'm not criticising. I'm not criticising work rates, all I'm saying is they set off and reported in May. This was starting in March. One of the fundamental problems we had in the whole of this is that you were fixed by the dates in which EAGA met, in terms of pushing the programme along, and in May they turned up saying, "We have got into the business of heat treatment." In other words, they turned up and said, "Abbott is hopeless," and so on and so on and so forth. "This is preliminary stuff."

And then the heat treatment thing tumbled out and my

understanding was -- because Abbott was causing a lot of trouble -- that they had to repeat all these samples again, but there were only 210.

Q. I think 220 was the figure perhaps, and they looked at some other samples as well but, as you say yourself, I don't want to split hairs over that.

Can we move on through the statement? You make another point about the whole programme being dogged by lack of financial support. The reference for that is actually a letter from Joy Atterbury, [SNB0050191], a lack of financial support from DHSS budget holders. Rather tantalisingly, we don't seem to have the chronology document. Perhaps that's something that we can obtain.

A. I have got it.

Q. You have got it?

A. Yes.

Q. Then we certainly can.

A. I'm sure it's available from the SNBTS. Yes.

Q. Yes.

A. It's very interesting.

Q. Yes.

A. My source, in terms of -- I mean, I managed to find a piece of paper that confirms my memories in terms of money. But my regular source of information concerning

the great difficulties our colleagues south of the border have in funding came from Harold Gunson.

Q. Right. I think what you are referring to is that passage in the letter:

"Minister of State, Health ... "

Which I think is the translation of the abbreviation MS(H):

"The Minister of State for Health declines funding for AIDS tests."

That's a decision in November 1984.

A. That, I think, is in the chronology.

Q. Yes.

A. I think it's pencilled in.

Q. Yes, but are you aware of any actual evidence that that had a bearing on the evaluation process?

A. No, I'm not aware of it at all but I am aware that there seemed to me to be -- the kits, as I understand it, for the evaluation process -- this applies also to Hepatitis C -- were purchased by the procurement directorate, or some directorate within DHSS, that purchased things, which I think is part of Supplies Division, and I am aware that were there were often difficulties negotiating with companies in terms of the price that was going to be paid and so on and so forth. So the delay of six months, some of it could have been

pure administrative delay and so the labs didn't have access to them for some time.

Q. Right.

A. That's really all I can comment on on that.

Q. Okay. Now, can we go back to the statement, please? You go on to refer to a comment at the May 1985 EAGA meeting.

A. Yes.

Q. That's actually not on my document list because I was hoping not to have to look at it but since you are saying that the other comment is in these minutes, we should look at them, if we could, please.

A. It was in the context of what we have just talked about. The chairman said, "Look, calm down everybody, there is no rush."

Q. Let's read what he actually said. [SNB00010365], please.

A. I should say this is what's minuted he said.

Q. Right. Were you there?

A. No, I wasn't, but I had a lot of phone calls afterwards.

Q. Okay. Let's just go through these, please, and firstly look at the comment at 5.2 that you quote. Dr Smithies is reporting on the evaluation so far:

"The chairman said that while it was important to introduce a reliable screening test as soon as possible, an effective evaluation of the tests was essential and

should not be rushed."

Now, you were ahead of me earlier, Professor Cash, but weren't you saying much the same thing?

A. Yes.

Q. "We need an effective evaluation but we don't want to rush if that's going to compromise the effectiveness of the evaluation." Is that it?

A. Well, you can hasten because you prioritise things higher up and you put in a resource to get answers as quick as you can.

No, the notion of rushing and therefore doing a second-class job was furthest from my thoughts, but I think the urgency -- I mean, this is repeatedly evident in the whole of this period. The urgency to resolve the issue as to what test then should we introduce -- there was very little urgency that I was aware of.

Q. Right, but I'm not sure that your comment that this illustrates an extraordinary laissez faire attitude is entirely fair, is it? Dr Smithies is reporting on the story so far. It seems redolent of everybody actually getting on with it and doing the best they can to have results quickly.

A. At the time I can assure you I didn't share that view.

Q. Right.

Can we go back to the statement, please? You go on to talk about the intervention of Professor Bloom. Now, Professor Bloom was at the meeting -- I'm sorry, I forgot; I should have allowed you to direct us. Can we go back to that meeting, please, [SNB0010365]. Can you direct us to the passage about the heat treatment having compromised the evaluation programme?

A. I would need ...

Q. Actually --

A. I'm struggling actually.

Q. What we will do is we will give you that over lunchtime, a hard copy of it, and let you have a look?

A. Yes, and if I got my own copy, I've marked it. Yes, thank you, that's very kind. Thank you very much.

Q. I think that would be fairer.

THE CHAIRMAN: I think that's 1 o'clock.

MS DUNLOP: Yes, we will come to Professor Bloom after lunch. I wonder, because there are some pressures of time, if we could perhaps start at ten to two. Is that ...

THE CHAIRMAN: I'm happy to start at quarter to two or earlier --

MS DUNLOP: Even better. Thank you.

THE CHAIRMAN: We will make it quarter to two, ladies and gentlemen.

MS DUNLOP: Yes.

(1.01 pm)

(The short adjournment)

(1.45 pm)

THE CHAIRMAN: Yes, Ms Dunlop?

MS DUNLOP: Thank you, sir. Professor Cash, we were just going to look at the point at which Professor Bloom commented on the progress, or lack of, in relation to the introduction of screening. Can we look, please, at a letter he sent, which is [DHF0025510]?

First of all, he wrote to the DHSS on 31 May 1985 and he is reinforcing views he expressed at the meeting of 29 May -- that's the EAGA meeting -- concerning the rapid introduction of screening. He says his anxiety is compounded by a paper from the Middlesex Hospital about the rising prevalence of HTLV-III. He thinks one or more of the FDA-approved tests should be introduced immediately to test donations.

He went into print more publicly. If we look at [LIT0010333], he wrote to the BMJ and his letter was published on 22 June 1985. We can see the heading, "HTLV-III, Haemophilia, and Blood Transfusion".

I suppose we should bear in mind, Professor Cash, that the situation for haemophilia clinicians in England at this point was not as good as it was in Scotland. Is

that right?

A. By a huge margin.

Q. Yes, because in Scotland we know that heat treatment had been introduced as from December 1984.

A. Plus they didn't have enough, so they had to buy.

Q. Yes. He talks about AIDS as the most important complication of haemophilia treatment and then he says at the bottom of the left-hand column:

"The prevalence is rising. The safety of cryoprecipitate and unheated UK blood products with regard to HTLV-III infection can therefore no longer be assumed."

Then he goes on to say in relation to blood transfusion, that:

"People receiving whole blood, platelet transfusions, cryoprecipitate or other blood derivatives from 50 or more donors in a short space of time, may be exposed to a risk of HTLV-III as high as one in 20 in certain areas of Britain. All these considerations underline the need rapidly to introduce screening. Three commercial test kits have now been approved by the American Food and Drug Administration. Although there may be a small number of false positives, it's unreasonable to delay testing until this possibility is eliminated."

I have been saying Professor Bloom but of course the co-signatories are Dr Forbes and Dr Rizza as well.

You were very unhappy at the publication of this letter.

A. Yes, I was. I have to say. That's an understatement, I think.

Q. In fact you wrote to him. Can we look at [SNB0132251]. So your first instinct was to write back via the BMJ. So you had drafted a letter to send to the BMJ, but you decided not to send it and instead to send it simply to Professor Bloom and Drs Forbes and Rizza. And the letter is [SNB0132252]. Perhaps we should just take a moment to read it ourselves. (Pause)

The major error of fact to which you refer is the one in 20 risk. Is that right?

A. It is that plus the sadness that we are not all working together to achieve the end that we all want. The sadness that -- he knew this -- that Arthur Bloom -- Arthur was on the board of BPL. He was very much part of -- I thought and a lot of people thought -- DHSS and so on, and I was pretty unhappy, not that he -- I strongly supported his concern but going into the public domain. And at the time -- I mean, it's a long time now has gone by. At the time I thought this is a direct attack on the UK transfusion services. And

here we were battling away with our colleagues in DHSS to get the kits evaluated quickly, to get them into use. But Arthur didn't seem to know that, nor did Charles Forbes, so I reacted pretty angrily and I was glad I didn't send it to --

Q. Right. You say that:

"FDA approval of the currently available HTLV-III antibody screening kits was obtained in a climate in which critical scientific analysis was of secondary interest."

Then you are correcting an assertion that UK health departments are delaying until false positives are eliminated.

A. That's nonsense. They weren't doing that. That's unfair.

Q. You think he was giving insufficient weight to the matters of confirmation or reference testing and the counselling of donors.

A. Very much so, and he doesn't come from that world. So I completely understood.

Q. Right. You say it was a highly complex and potentially -- we can read over -- explosive problem. Right.

Professor Cash, I should have given you the opportunity just to direct us to the passage in the

minutes of the EAGA meeting in May that you want us to look at. Can you tell us what we should read.

A. I'm certainly due an apology. The only reference -- I sensibly had a look at the July. I got your colleagues to let me see the July, and there is the reference of heat treatment there on 7.2, the July --

Q. Right, this is the July minutes?

A. Yes. But can I just, before you may feel you are wasting your time. I have a document -- and I have got to get home to my study and pull it out -- in which the issue of heat treatment became -- this is way before July -- became an issue to the extent that in my document it says that Abbott had lodged a complaint saying that, "Our tests were not developed and designed for heat-treated sera."

So it was an issue and if you wish, I will get home and dig that out for you. Clearly I have not referenced my document appropriately and I just apologise.

Q. Right. Just so that we have the position --

A. The July has got -- they are saying in July, "We are recommending these three kits, (a) because of the low rate of false positives, (b) they give reliable results with heat-treated sera, irrelevant to us. Okay? So it's still there, and if you look in the Lancet publication of this final work, the question of heat

treatment is not contentious, they just declare that they have done it.

Q. Right. So --

A. The diagnostic lab, very important.

Q. Just to be clear, neither in the minutes of the May meeting of EAGA nor in the minutes of the July meeting is there a specific reference to the use of heat treatment in the evaluation process having derailed the exercise and necessitated repetition?

A. I need to get that for you.

Q. So you think that's in a third document which you are going to dig out?

A. Yes, I need to -- I don't like the word -- a lot of my information was coming up, in all of this, from Dr Harold Gunson.

Q. Right.

A. I don't like to use the word "derail", which you can understand. They had to repeat so that when they came, in their final publication, and when they delivered what the department wanted, advice to BTS, it was on samples that were unheat-treated.

Q. Yes. We have the July minutes and the May minutes but you don't think the actual reference you are looking for is in there?

A. No, I can see it in my mind's eye. If you would bear

with me, I would be very happy --

Q. Indeed. We would be very grateful to get that from you.

Can we go back to Professor Cash's statement, please? That's [PEN0171038].

We have dealt with the concerns that were raised by Bloom, Forbes and Rizza. Can we move on to 2.10? You think that the publication of the letter in the BMJ speeded things up? It's perhaps rather difficult for us to form any kind of judgment as to rates of progress at this remove, but you go on to say that:

"Appropriate UK BTS evaluations were not undertaken."

And the only evidence you have that some form of RTC evaluation was done appears in a meeting minute from January 1986.

Professor Cash, there is a document which I need to put to you, which is [DHF0029428]. We had this in our database dated as 7 May 1985, and if we look at the top right-hand corner, please, we can see why we did, but it can't be because it goes on to describe testing carried on into August 1985. But this looks to be the first draft of the report of phase 2.

A. Yes.

Q. Doesn't it?

A. It does on the face of it, the notice -- it's coming

from England exclusively.

Q. Yes.

A. Yes.

Q. I see that.

A. Yes.

Q. But it is what we have been calling "phase 2". So it's the field test. It's once the phase 1 testing had been completed and the two kits in particular had been recommended as suitable for use in the Blood Transfusion Service -- that's the Organon kit and the Wellcome kit -- that information having become available at the end of July, phase 2 was commenced. If we perhaps just look at the whole of the front of that and then into the summary, in fact it says:

"There is evidence of problems affecting both test kits and testing centres, which would be expected to be reduced with increasing experience."

Does this really echo what Dr Dow said in his paper, that a substantial number of plates failed to meet the manufacturer's values for quality control parameters?

A. Possibly. Honestly, I can't from this -- I'm just horrified to see -- it's the first time I have seen this:

"Failed to detect a strong positive sample."

I'm not sure what that means.

Q. I think we misled ourselves by going with this date. I think it may even have been a date that the Department of Health had for the document, but once you appreciate that it's not a document from May 1985, everything falls into place.

Can we go, please, to page 5 of this? That's our page 5, not their page 5. Thank you.

There we are. 6,160 samples?

A. That's more like it.

Q. Yes. Carried out at Manchester and Edgware.

A. I know that, yes. Professor John Barbara would have been masterminding this, I suspect.

Q. Right. There is a more useful table earlier in the document. I think it's page 4.

Yes, there we are.

A. Yes.

Q. In fact we will understand it better, I think, if we go back. That's table B and then table A, I think, is on the previous page, obviously. Then back to the text immediately before that. One more page back, please. Yes, it's that:

"The Vironostika kits failed frequently to detect the weak positive sample."

That's Organon:

"In both test centres, the Wellcozyme test failed to

detect the strong positive sample on several occasions."

Can we look at table A on the next page?

So some false negatives.

A. I have to confess, I know I'm old and decrepit but I have not seen this and I have no recollection of seeing this, and my reference to this was in fact a minute in the NBTS directors' meeting.

Q. Yes, which is not as good.

A. It's a one-liner and so on. But was this published? I mean, it must have been a huge moral obligation to publish it, I would have thought.

Q. Well, I don't know what happened next. Certainly from Dr Dow's comments it does appear that even having chosen the Wellcozyme test, there were problems in the first few months.

A. Brian talks specifically about problems of specificity, in other words screen positives. A higher rate than originally they had seen in the -- reported by Philip Mortimer and Richard. When they got the sort of production kits -- this is normal -- they found they are not as good and so on, but they required to do a little bit of tweaking to get it right. But this was about false.

Q. False negatives?

A. False negatives, I beg your pardon, this is for false

negatives.

Q. Hm-mm.

A. I mean, has it been published, I ask? Do you know this?

Q. Not off the top of my head. I'm not very clear what happened to this, if there was a final version of the results. There must have been, I guess.

A. I can imagine Wellcome would want it buried.

Q. Well ...

A. I'm very surprised. It's a very good kit, and these were all -- I'm sorry to go on. These were all confirmed Western Blots, were they? So there was no doubt about the positivity. I'm sorry, I'm asking details.

Q. Yes, I think these were, as it were, the controlled sera. Let's go back to the page before. We see the calculation of the results:

"It's apparent that minor alterations to the method of calculating the cut-off value for the Vironostika kit would lead to the detection of almost all the weak positive coded control samples, but the failure of the Wellcozyme assay, with one batch of kits to detect the strong positive coded control samples, could not be remedied by simple adjustment of calculations."

A. I would have to say these results contrast pretty strongly from those published in the Lancet from

Philip Mortimer and Richard Tedder's studies. They really do. And just on the face of it, I can't -- I wouldn't wish to comment any further.

Q. Right.

A. I mean, the Scots were the team -- we are the team that, you know, took a view, if you have an elephant in the room, anybody can detect that blindfolded; just put your hand out. The real question is, if in fact it's a wee mouse, is your technology good enough? And we developed -- and NIBSC in London did it all for us, it was wonderful -- the notion of "Tricky Dicky", we called them, in every run that was done, every day, and it's now going on today as routine. Every run, every day, you put in coded Tricky Dickies, and if the assay doesn't come up and pick them up on that day with that batch or whatever it is, you scrub the whole -- you have to go back again.

But here the impression from this paper is they are missing elephants. I find that very difficult.

Q. I mean, the Philip Mortimer article, which appears in the Lancet in October 1985, relates to phase 1.

A. That's phase 1? That's right.

Q. Yes.

A. And I don't think they report any false negatives, Philip Mortimer's team.

Q. Well --
A. I don't think.
Q. I think they do actually, just from looking through the article, but perhaps we should leave it. I don't want to try and ascertain, from researching at the moment, what happened to these particular results, but it does looks as though what was being said was that there was one batch in the Wellcome test which had failed, which might, I suppose, be slightly more comforting than if it had been a selection of kits from different batches.
A. But even then that raises, for the manufacturers, serious questions about their quality assurance and batch release.
Q. Yes.
THE CHAIRMAN: Professor, I think I have to know very briefly what a Tricky Dicky is.
A. A Tricky Dicky is a strong positive.
THE CHAIRMAN: I thought that's what it was.
A. You serially dilute to a point where you can't detect it any more. You pull back one and you can still just detect it. That is a Tricky Dicky.
THE CHAIRMAN: And that provides you with a particularly critical test of the effectiveness of your testing?
A. Your accuracy, the reproducibility, the sensitivity of your test.

THE CHAIRMAN: Yes. As Professor James says, on that day.
MS DUNLOP: Oh, yes.
A. Yes.
Q. Professor Cash, if we can go back to the statement, I think you were suggesting that the phase 2 was not done but it does look from this document as though it was.
A. Yes.
Q. It wasn't complete before the screening tests were --
A. Yes, and it wasn't done in Scotland. I hesitate to push that, but, you know -- but my information comes from an NBTS directors' meeting minute, and it was very brief. It was way past its sell-by date, ie in 1986, so it had all happened and so on.
Q. Yes.
A. I wasn't aware of it. And I would very much like a copy of this, if I may?
Q. I'm sure that can be furnished.

Can we move on to paragraph 2.11, please? You are talking a bit about funding. This is a little more than half way through the paragraph:

"SHHD used the position that they had agreed that there would be ring-fenced funding to ensure that it retained, through the CSA's finance branch, control of the start date of testing."

Can we look at that letter, [SNB0057915]? This is a letter from you to Mr Davies, I think.

A. John Davies, yes.

Q. In the SHHD. It wasn't immediately obvious to me, professor, how you took from this that SHHD were trying to control the start date.

A. Oh, can I -- would you like me to help?

Q. Yes, please.

A. The start date will be determined by (a), government and (b), the release of money to purchase kits in advance of that start date. What became very clear to me, really after that unfortunate chat with Archie in January -- Archie McIntyre, was that quite rightly, the department had the facility to instruct, which it did -- I eventually discovered -- instruct the CSA that it was not to release money for the purchase of kits. And that was really pretty straightforward to implement that -- until such times as the department in fact informed the agency that that is what was to happen.

I didn't get any communication along this line but I regularly -- about on a monthly, maybe two-monthly -- had an meeting with the treasurer -- his name was John Morrison -- with John, and on one occasion he pointed this out to me and also pointed out -- you know, it was a very serious matter -- that under no

circumstances were we to start purchasing kits until such times as SHHD had pulled the trigger.

Q. Right. It rather looks from this letter, professor, though, it's almost to work in reverse: that there will be an agreed start date and then people will work back from that and say, well, by, as it were, T minus 3 we have to have purchased such and such a piece of equipment, and by T minus 2 ... and so on.

A. Train the staff and put all the counselling in and all that.

Q. Yes.

A. Yes, absolutely right.

Q. It doesn't look as though anyone was actually trying to influence the start date by withholding money. But that's not what you are suggesting.

A. No, I'm not even suggesting that.

Q. No.

A. Because when we come to, as you well know, Hepatitis C, certain individuals jumped the gun and started before the rest of the UK.

Q. Yes.

A. You already, I think, have got information here that, you know, directors were very jittery. Who had the duty of care and so on and so forth. And this was a very lively issue and it emerged very big in Hepatitis C.

Q. In fact with HIV testing too, Newcastle started.
A. I didn't know that.
Q. Well -- we have documentation about that.
A. And I have a letter from Harold Gunson in which he is throwing in the towel and saying, "You think you have got problems".
Q. Can we --
A. We didn't have those problems, though, not in a big way.
Q. Can we go back to the statement, please?

You mention some of the problems south of the border and you say that your view is that in England routine testing was commenced without an agreed strategy for confirmatory testing. I do not think we need to go into that. Then at 3.01, you talk about:

"The establishment of the SNBTS microbiological reference centre in 1989."

Which is a subject we intend to explore in our C4 topic. Then section 4 is about alternative testing sites.

A. Can I just interrupt and say you have skated over -- and I'm very happy with that -- some very important conclusions that SHHD did make.
Q. Some very important? I'm sorry, I didn't catch that.
A. Contributions.
Q. Contributions?

A. That SHHD did make a great difference in terms of funding these various things, compared to our friends south of the border. Can I just make that point.

Q. Thank you.

A. You may think I have been totally and 100 per cent critical of SHHD. It's not so.

Q. Thank you, professor.

I think the only reason I was skating over it was because it seemed to be about England, and at the end of the day we are not investigating the detail of what happened, but I take your point that there might be a contrast to be made:

"Alternative testing sites ..."

I did seek earlier to draw attention to the fact that that was actually mentioned as early as 16 July 1984 by Dr Ian Fraser. So you, I think, seemed to be saying in this section of your statement that it was the FDA who made this point about worried citizens turning up as potential blood donors simply to get an AIDS test, and that seems, from the documentation, to have been appreciated as a risk in the United Kingdom, certainly from at least July 1984.

You go on to say that:

"SNBTS directors persuaded their NBTS counterparts to join with them in exhorting the UK health departments

to instruct regional health authorities to establish alternative testing sites."

I should say that reference is your Lancet letter of March 1985, which we don't need to look at again but you make the point in that letter about the need for alternative testing sites. Then you say:

"More direct contact on this topic was made through EAGA."

Can we look at [SNB0010430], please?

Just to note -- because we are slightly short of time -- that this seems to be making the point that people who want an AIDS test may need to be able to get one without having to go through either their general practitioner or a sexually transmitted disease clinic. Yes.

Can we go back to the statement, please?

You mention to us further letters of concern on this point, which again I don't want to go to because they are slightly beyond the period we are examining, about the effectiveness of these sites, but just for the record, [SNB0132889], which is mentioned, is dated 31 July 1987 and [SNB0132892] is dated 14 December 1987. They are letters from you to Dr Ian MacDonald as chief medical officer.

Right. The specific questions, and on a number of

occasions you say that you have really already answered one of our questions in your introductory section. I don't think we need to go back to your letter to Dr Bell of 24 January.

On to the next page, if we could, please.

You have given us your answer about the letter of 24 January and then the letter of 25 January, you to Dr Mitchell, we have also looked at.

A. Yes.

Q. Can we go on to the next page, please? In relation to the introduction of screening in Scotland, you say:

"Most of the answers can be found in [your] background notes."

Then you refer to the February 1984 letter to SHHD. That's the applied research letter.

A. Yes.

Q. If we can call it that for shorthand. You do say that:

"Even if SNBTS had been going it alone, there would have been a prior assessment of local data on specificity."

So you wouldn't just have gone into testing by buying a kit and starting immediately?

A. No, it was ethically unacceptable.

Q. Right. Then question 13, we focused on a meeting of the SNBTS coordinating group on 19 February 1985, and this

is really returning to the theme of what happened when assessments in the West of Scotland had been mooted and something of the nature of cold water had been poured on the idea by Dr McIntyre, as you say.

Can we look at [SNB0039171]? This is the coordinating group meeting on 19 February and can we look, please, at page 6? Can we scroll down a little bit, please?

There is a discussion about the commercial test kits possibly being insufficiently accurate, a reference to the working parties, a need for cooperation between SNBTS and consultants in infectious diseases, and then on to the next page, please. The topic of screening is discussed particularly at (e):

"Growing concern about the number of false positives produced by the current generation of tests. All available kits were to be evaluated under DHSS sponsorship."

Then the section in bold:

"After a full discussion, it was agreed that Dr Cash's letter of 25 January to Dr Mitchell should not be pursued at the present time."

And that's linking back to the section in your introductory notes, which we have --

A. Can I add a footnote that one of the wonderful things

about working with Miss Morag Corrie, who was our national administrator, and she did the minutes at the director's meetings, is that it's well coded. If ever you see after a full discussion -- and it appears sporadically over the years -- the prisons one, that was a full discussion -- you can be sure there was heat generated and there may have been criticisms of the department and whatever. Morag would never record those. It was a "full discussion". And I can assure you that's quite a lively debate that took place.

Q. Do you remember it? Do you remember that meeting?

A. I remember this issue because -- it's not surprising because I felt in my heart we should get on and move on, and pulling out after that chastening conversation with Archie McIntyre hurt a little.

Q. Well, you did get a letter --

A. I think I chaired this bit, is that right?

Q. You were in the chair for item 6 onwards?

A. Is that 6?

Q. Is that 6? Let's check. Yes, it looks like it.

A. That's a relief.

Q. Can we go back to the first page, please? Yes. You chaired 6, 8 and 9, so you were.

A. Yes.

Q. Right. You did get a letter back from Dr Bell. That's

[SGH0027260]. 6 March. Dr Bell is saying he is grateful to you for telling him about the decision to hold off from validation of kits until protocols have been agreed through EAGA. So perhaps another slightly different slant, not necessarily, "Don't do it at all," just, "Don't do it until we have some protocols agreed".

A. Maybe. I honestly -- it's so far back. I notice Bert says it is to be welcomed. I imagine a great relief. I imagine.

Q. They were pleased?

A. Yes, yes. It fits in with the conversations I had had with Archie.

Q. Right. Can we go back to the statement, please, [PEN0171038] at 1044, then on to 1045? The next question that concerns the preference for a radioimmunoassay. Again, we have covered this to some extent already.

A. I think so, yes.

Q. The question of RIA versus ELISA and also whether BPL were to have a manufacturing role. I thought I would just show you the suggestion that the Department of Health were not terribly keen on that idea, which I think fits with your evidence earlier. That's [PEN0121938]. This is actually Dr McClelland's report of the meeting of 27 November 1984 but if we look at

page 2 of this, it's actually Dr Lane who is quoted by Dr McClelland as pushing to produce test kits.

A. Yes, sure, that fits, that's correct.

Q. Is it? Right:

"Lane is pushing to produce test kits if bulk inactivated antigen is provided. The departmental enthusiasm for this muted."

So not very keen?

A. Yes.

Q. Right.

A. As I have said, the department were under some political pressure not to permit this development, because you get into the private sector and the marketplace.

Q. Can we go back to the statement please. You say that you thought at the time that the chance of the proposition, whether it came primarily from Dr Gunson or Dr Lane, being accepted by ministers was remote.

A. That's what I felt, yes.

Q. Right. Then in the next question we talked about the CBLA meeting on 1 February 1985. Can we look at [DHF00030219]. Can we just scroll through this, please?

A. There you see Arthur Bloom on the board.

Q. Yes. And Dr Gunson. Mr Smart is in the chair and he is responsible for a slightly delphic comment.

Sorry, you were going to say David Smart?

A. Yes.
Q. Who was he?
A. I think --
Q. Or is he?
A. -- you know, the classic in that area: bring a businessman in to sort out the public sector. David was someone from, I think, the pharmaceutical industry.
Q. Can we look at the next page?
A. There is Duncan Thomas referred to.
Q. Yes, NIBS and C.
A. Yes, as is Jerwood (?).
Q. Yes. Then, further on, the next page, we are still in "matters arising", a discussion about logos, plasma supply. We find the comment, a discussion of screening.
A. There we are.
Q. Yes. Is this the section you were thinking about when you spoke earlier about the CBLA?
A. Yes, I think so.
Q. Yes. BPL could produce the test.
A. The chairman stressed that revenue sparing was as important as saving.
Q. I find that slightly hard to follow?
A. Harold convinced me that they were very enthusiastic ...
Q. Dr Gunson not feeling very positive about the need to convert the United Kingdom for enzyme testing.

A. You know, he was like me, an old man, past his prime in terms of science, but I have to tell you -- we can now look back and we were right. The world was moving on very rapidly.

Q. Was it old dogs and new tricks?

A. That's right. Isotopes, you know, they are potentially dangerous, and really they were sweeping them out -- health and Safety -- out of labs as much as they could, they were retained in super-research laboratories, we had one. But for routine testing of thousands of donations a day, you know ...

Q. Right. So Dr Gunson --

A. It wasn't magic, it was inevitable really.

Q. Right. Back to the statement, please.

Professor Cash, you explain much of this reasoning on page 1046, and having noted Professor Zuckerman's contribution, which was more in favour of thinking seriously about ELISA tests, you say that if Abbott had moved from RIA to ELISA.

A. Which they were doing.

Q. Which they were doing, that was significant. And you point out that Abbott were also at an advanced planning stage to replace their Hepatitis B RIA with an ELISA assay.

A. Which had been, as I said before, an absolute winner.

Q. Yes. And similar developments were believed to be underway in another major company. Can we just glance at that? [SNB0130724]. That's Dr McClelland to you, 14 March 1986. This just narrates the evaluation of ELISA tests for Hep B. Interesting to see that, for example, DuPont expect to be in the marketplace very soon with a pair of ELISAs for HTLV-III and Hep B. It does look as though Colindale were carrying on with their business of evaluating test kits.

A. That's John (inaudible).

Q. Yes. Right. Back to the statement, please, and on to the next page.

We were still asking you about this switch from RIA to ELISA. You talk about Dr Walford managing the liaison with Wellcome Diagnostics. In fact, without looking at it, just to give the reference for the document that demonstrates that Dr Smithies took over Dr Walford's duties, that's a meeting of 6 March 1984, the reference is [SGH0070734]. Then the secret meeting, you have given us your views as to what that might have concerned, but you say you do not have any particular information about it.

Then on the next page, the introduction of screening in Scotland. We had said that the working party of the regional transfusion directors' committee produced

a report on 11 July 1985. It's interesting to look not at the reference that is supplied in your statement because that's the corrected minutes; it's interesting to look at the original report, which is [SNB0010357]. We can see who was on this committee. It included Dr McClelland.

A. Yes.

Q. But it's item 3 that we are particularly interested in. Originally the report said that routine screening tests for HTLV-III antibodies shouldn't be introduced until the following had taken place. Now, 2 and 3 are straightforward:

"The establishment of reference centres and the establishment of alternative venues for testing."

But:

"1. Until the proposed evaluation in the NBTS of different test kits has enabled satisfactory systems to be selected ..."

However, there is a corrigendum, which is [DHF0017532]. We will ask Dr McClelland about this but the idea that the NBTS evaluation would be completed before the introduction of screening has been departed from because the corrigendum says that:

"There was a degree of urgency for the introduction of routine anti HTLV-III screening of blood donations

which precluded the completion of NBTS evaluation of different test kits prior to arrangements being undertaken for the introduction of routine screening."

So by this time the two are happening, or it's intended that the two will happen together, that the NBTS will be evaluating and local directors will be making arrangements to obtain kits.

A. Yes.

Q. Yes.

A. And told to do short contracts because there may be some problems we need to change.

Q. Yes.

A. So the full phase 2, in my view, I think I have said, was not done.

Q. I accept it's a slightly loose end but we have looked at a draft which certainly seems to relate to --

A. If that draft had come into the real world, I can tell you there would have been a phase 3 and 4, I think.

Q. Well, as I'm sure we will see when we look at Hepatitis C, these exercises tend to be bedevilled by the introduction of second generation tests and so on, so no sooner have you finished your complete evaluation of the first generation test than the second generation test comes on the market.

Can we go back to the statement, please. You

advised us to get in touch with Dr Dow and we did, and we will look at his statement in full later this week. You say:

"There is a lot of associated and dedicated specific equipment which comes with use of the kits."

And we don't doubt it.

Can we go over on to the next page, please? Just for the record, I don't think we will bother going to it but that reference, [PEN0170653], is the Mortimer article in the Lancet in October 1985.

That's [PEN0170653] and it, sir, as I said earlier, deals with the first phase of the evaluation exercise and then we pointed out about the corrigendum. It does look, Professor Cash, as though in July 1985 the transfusion directors, including Dr McClelland, on that working party or that group felt that it was not going to be possible to complete the second phase, the field evaluation, before introducing testing.

A. Yes.

Q. Yes. So you say:

"DHSS saw the UK BTS component of their evaluation programme as a low priority."

But we should note that the recommendation not to wait until that stage was complete actually came from a group of transfusion directors.

A. Yes, I think they were concurring with the message that Dr Smithies brought to them.

Q. We went on to ask you in question 20 about the statements in the report about the other steps necessary. Both the report and the corrigendum stated that the other steps necessary before the commencement of screening were:

"... that reference centres had been established and that alternative venues for non-blood donors to obtain testing had been established."

I think, with respect, in your answer you may have misread the question because you say:

"I don't believe it's correct to conclude that in the report or corrigendum the authors state that reference centres and alternative testing venues had been established."

I think that's actually not what the question says. The question says that these were identified as pre-conditions for the introduction of screening.

A. I beg your pardon.

Q. Yes, and that is right?

A. You are quite right.

Q. Thank you. I didn't write the questions. So I'm not clarifying for my own benefit.

The position about alternative testing venues we

would intend to explore with Dr Scott. We have a deal of documentation on that topic. We should be able to ask Dr Scott about that but we note that the need for it was identified in 1984, and you yourself wrote to the Common Services Agency in fact on that matter. Just again, without going to it, the reference for that is [SGH0027266] and then [SGH0027267] as well. The first stage of the evaluation was completed on 30 July and then screening was introduced on 14 October.

Can we go over onto the next page, please?

You make the point that one device that was used to avoid long-term commitment to kits, which might turn out to be less than the best available, was to enter into short-term contracts. You say:

"Problems of poor specificity did not subsequently emerge."

But I think both that document we looked at, the first draft of the phase 2 study, and Dr Dow would suggest that there were problems --

A. Absolutely right. I have marked it here. That's not strictly accurate. That was in hindsight.

Q. Right. Then just on to the next page, you say that it might have been possible -- or it would have been possible for a short-term contract to have been entered into at an earlier date. You say:

"Yes, given that we were permitted to satisfy ourselves that kits available had acceptable specificity and that Scottish ministers would allow the SNBTS to go it alone ..."

So again, you are saying you wouldn't have done it without an evaluation.

A. Yes, that's correct.

Q. I suppose the other factor we have to build in is the practical one about the need to actually have your hands on some kits.

A. Yes.

Q. Yes.

A. And counselling and -- I mean, you know, it's a big package.

Q. Yes, we are talking about a short-term contract but, yes, counselling and alternative testing centres.

Then we asked about the reference centres and Dr McClelland has given us some more information about that, the reference centres for Scotland being Ruchill in Glasgow and the clinical virology laboratory in Edinburgh, Dr Peutherer. Is that right?

A. John, yes.

Q. Yes, you say that, sorry. And these were up and running by 14 October. A small piece of the jigsaw was missing because we weren't sure what tests were chosen for the

West but several people have told us it was the Wellcome test.

Then the letter to the Lancet. That reference actually, I think, is a draft of the letter, which appeared in the Lancet in March 1985.

A. That's correct.

Q. We have looked at that and you give the reference in your answer, [LIT0010374]. In fact, that's the extract that includes both the Carlson letter and your letter.

A. Yes.

Q. You do say that in late 1984 the kits that the FDA had recently looked at had screen positive rates of only 1 per cent in a low risk population. I think, in fact the screen positive rate, if that's the first round, was about 10 per cent.

A. In the early days, yes.

Q. Yes?

A. But I was getting messages from John Petricciani that they may be getting it down to 1 per cent now, and that was very encouraging. And I think Ruthven and the boys eventually said, "No, it's half a per cent".

Q. But that reference is the MMWR publication and perhaps we should just look at it again. [SNB0049195]. The bottom of page 2, please, then on to page 3.

This is talking about high false positive rates.

I think we have to bear in mind, do we, that the screen positive rate is the first round?

A. Absolutely.

Q. Yes.

A. That's when you are told, "Bye bye, you can't be a donor".

Q. Yes, I don't think that there is any reference here to the screen positive rate being as low as 1 per cent. That might have been the confirmed positive rate which is different from -- that's round 2. That's after Western Blot or some other kind of confirmatory testing?

A. But we have looked at data. If you look at the overall data that came out of the NBTS study, the screen positivity is very low indeed. The problem was they were missing some raging positives, and my understanding is that our screen positive -- I need time to check but I'm fairly sure there is information that the initial screen positive stuff that really concerned us was 10 per cent and then we got information from the States -- and this was verbal, and I thought it was eventually published and I will need to chase that -- that the newer kits look as though they are now much more like screen positive 1 per cent.

Q. I think the Carlson letter was the one that --

A. Yes, I think that was us bawling and shouting in the

early period.

Q. That's the March 1985 letter?

A. We eventually got from John signals that, "Hey, it's looking much better".

Q. Right.

A. And that's interesting from the States because that was the Gallo preparation, if you bear in mind Robin Weiss's comments.

Q. Yes. Can we go back to the statement, please? We are now at 1054. You are talking about the intervention of Professor Bloom and directors Forbes and Rizza, and we have really covered that. This is item 28. We asked some further questions about the false positive rates and the belief that seems to have been held in the spring of 1985 that there were high false positive rates.

There certainly does seem to have been some information that there were significantly high false positive rates in some of the American tests --

A. Oh, yes.

Q. -- in early 1985, and we will look at that later this week.

I suppose, professor, that if you are telling us there was other information that said, no, the false positive rate is not that high, one would just end up

with a confused picture because you wouldn't know on which source to rely.

A. I think I declare that I was confused, that there were very conflicting messages coming out of the States, and there were all sorts of commercial pressures. My mates over there saying, "Don't believe the half of it, John, it's all Abbott versus whoever, versus whoever". Hence our concern in January 1985 that we really must take a grip, the Brits must take a grip and do the job negatives in their own population.

Q. Yes, the "job" being the evaluation?

A. Yes.

Q. Then just looking at your response over on to the next page, please. I'm sorry that I'm rushing you slightly, Professor Cash but we do have a booking to see Professor Weiss at 3 o'clock.

A. I was late this morning, I apologise.

Q. No, it's not that. Professor Weiss has managed to find us some time even though he is leaving the country today, I think not in connection with us, and he is only available for an hour between three and four. So I think what we are going to have to do, if you don't mind, is ask you to sit and listen and then we will resume at four, but I'm hopeful that we can perhaps just finish this just now.

You go on to conclude with some remarks about concern about false positives. You say that:

"A figure of 10 per cent would have been a cause of concern."

Then you go on to tell us about some of the contacts you had in the United States. Then finally we asked about the possibility of introducing some form of testing on a more limited scale, that being a possibility alluded to by Dr McClelland in his letter to Mr Madden of Wellcome, and you suggest that we should ask Dr McClelland, which we will gladly do on Thursday, I hope.

Excuse me one moment. (Pause)

Sir, we have managed to make it.

THE CHAIRMAN: Yes.

MS DUNLOP: It is just coming up for five to three. I gather we need five minutes really.

THE CHAIRMAN: I'm anxious also that the stenographer should have a break.

MS DUNLOP: Yes.

THE CHAIRMAN: But this sounds a good point at which just to rise.

MS DUNLOP: Unless I think of more questions -- and it is perfectly possible -- I would hope that that concludes my questioning of Professor Cash. So if he is able to

stay to four, then it will be over to others to ask him such questions as they feel they must.

THE CHAIRMAN: I'm not going to comment on the capacity of any of us to dream up other questions, Ms Dunlop. We will simply rise just now.

(2.55 pm)

(Short break)

(3.05 pm)

THE CHAIRMAN: Professor Weiss, I had better get the right spectacles on, so I can see you.

I understand you wish to affirm?

A. Yes, please.

PROFESSOR ROBIN WEISS (affirmed)

Questions by MS DUNLOP

MS DUNLOP: Good afternoon, Professor Weiss.

A. Good afternoon.

Q. Good afternoon.

We have some questions for you. We are very grateful to you for making yourself available at such short notice and we appreciate that you want to be away at four. So I shall try to be succinct.

One of the consequences of arranging this at short notice is that we don't have a CV for you. If you were able to let us have one at some point, that would be enormously helpful but for the moment we can design you

as Professor Robin Weiss, emeritus professor of viral oncology at UCL Medical School. Is that correct?

A. That's correct.

Q. As you point out in your letter, which you sent to us, you are not medically qualified but you are a --

A. That's true.

Q. You are a virologist. Is that how you would prefer to be described, as a virologist?

A. Yes, most of my career I was a cancer researcher but I have been working -- studying viruses for almost my whole research career.

Q. Right. Thank you.

A. I have a PhD, not a medical degree.

Q. Thank you.

I want to ask you both about the letter which you have provided to our Inquiry, which is dated 12 September 2011, and also some questions which we sent to you on Friday, to give you some advance notice of where we were going to go. I think perhaps, if I could start just by asking you the first of the questions. Do you have various bits of paper in front of you?

A. Yes.

Q. Right. I said that we had extracts from a book by John Crewdson. It's the book entitled "Science Fictions", with which I expect you are familiar. One

extract in particular discusses your work in 1984. We sent pages 188 to 189, and our internal Inquiry reference for that is [PEN0170568] at page 25. So if you will bear with us, we can call that up on our screens and you have the hard copy.

This passage, which begins:

"In fact there was a British AIDS test ..."

That reference to your having developed an ELISA and used it to test nearly 2,000 Londoners is wrong, I take it, is it? It was a RIA not an ELISA? Is that right?

A. Your interpretation is correct.

Q. Thank you. Is there anything else on pages 188 to 189 that we should correct?

A. Erm, well, I read the whole book when it first appeared in 2002, the author kindly sent me a copy, and I would say everything, including these two pages, that he has in double quotes is meticulously quoted. Where he is not quite correct is that he attributes emotions to the people he is speaking about. So, for example, near the top of page 189 in this short excerpt, the beginning of the paragraph, four lines down, he writes of:

"... an angry Robin Weiss responded to the American rebuff."

I don't recall being particularly angry and I would not have called it a "rebuff". So those are the

author's interpretations, and throughout the whole book here, little nuances that are continuously changing the reader's impression.

Further down in the same paragraph he talks about, Burroughs Wellcome. That's the American company. The company we were dealing with was Wellcome Diagnostics Limited, which was not directly connected to Burroughs Wellcome. He says in the next paragraph:

"Amersham turned to Abraham Karpas..."

In fact Amersham first turned to me but unlike Dr Gallo, I felt that we could only deal with one company and I told them that there was another independent isolate in Cambridge and that they should follow this up.

So these things are not quite wrong but this particular writer has a style of writing and a motive behind his writing that I don't regard as authentic scientific history.

Q. Right. He was, as we understand it, a journalist with the Chicago Tribune. Is that correct?

A. He was and his first article that appeared either in late 1989 or early 1990, a huge piece for a newspaper article. It was about 9,000 words long. In my opinion that was much more accurate than this fat book of 500-pages.

Q. Right. I think we actually do also have the pieces from the Chicago Tribune, so perhaps we will go back again and look at them?

A. Okay.

Q. If we could go back, please, and look at the questions again, we said in question 2 that we were sending you some pages from an MMWR, Mortality and Morbidity Weekly Report, dated 11 January 1985.

A. Yes.

Q. And a particular comment there had caught my eye. We have seen it a number of times today but it's the statement at the foot of the second page that:

"The rate of false positives in a population where the incidence of infection is low will be high."

Perhaps, for our reference, if you could bear with us, we will just get that in front of us too. [SNB00491195], please.

A. I haven't quite found it on page 2. Could you point me towards it?

Q. It's at the very bottom:

"When the ELISA is used to screen populations in whom the prevalence of HTLV-III infections is low, the proportion of positive results that are falsely positive will be high."

A. Yes.

Q. Yes. Can we come back to that because I wondered if I could, please, go through some basics of sensitivity and specificity.

A. Hm-mm.

Q. Just to confirm that I have some sort of working understanding. The first question I wanted to ask was in relation to the concept of surrogate testing. These are just general questions because we are also going to be looking later in our Inquiry at Hepatitis C. So having an understanding is helpful to us. But if you talk about surrogate testing, we understand by that a situation in which it's not possible to test directly for a virus, so one tests for something which is thought to be found in association with the virus, as it were. And an example of that might be ALT, raised ALT as a surrogate marker for Hepatitis C.

The problem, as I understand it, with that sort of testing is that some patients will have raised ALT for other reasons. And we understand that some of those reasons might include being obese or having a high alcohol consumption. So if the testing was carried out on a population of skinny tee-totallers, it might be more reliable than if it was carried out in a population where people were obese or consumed a lot of alcohol or both, and that problem, I think I would understand, to

be inherent in the choice of marker. Is that a reasonable summary?

A. Yes, I think that's a very reasonable -- a marker such as ALT, which is a raised level of substance in the blood produced by the liver, is a general marker for inflammation of the liver or malfunction.

So it could be through excessive use of alcohol. It could be Hepatitis C, it could be Hepatitis B. It could be something else wrong with the liver. So it would indicate to a physician: you had better investigate liver function.

In the case of the antibody test for HIV -- would you like me to go on?

Q. I wonder if I could run some further thoughts past you on that?

A. I'll pause, I'm sorry.

Q. I understand that there is a difference in kind when we move to talk about testing for antibodies to HIV. One is still not testing for the virus itself, so I wonder if, strictly analysed, it's still a marker or is that a wrong use of the word "marker"? It's not a direct test for the virus, a test for the antibody. Is that right so far?

A. Yes, it is correct. I must admit I have never thought of it as a surrogate test but the way you describe it is

spot-on. That is correct. But it is a marker that is a very specific response to infection with that virus.

Q. Right.

A. So antibodies to HIV will be different from antibodies to influenza or to some other virus.

Q. And it's a good marker, as I understand it, because as we have learned in our hearings about HIV, unlike other viruses, you do not find people who have antibodies because they have cleared the virus. If they have the antibodies, they have the virus. So it seems --

A. Yes.

Q. -- it looks to be a good test?

A. It looks to be a good test. There may be a small number of individuals who have been exposed to the virus, who have not genuinely become infected, the virus is not growing inside their body, who nevertheless produce low levels of antibodies to it. There is a suspicion that some prostitutes in Africa, who are not infected, may be antibody positive, but they are very low levels of antibodies and also they do not recognise all the components of the virus.

Q. Right. I'm still tiptoeing here, Professor Weiss, but with antibody testing for HIV, the problems, as far as I understand them, seem to have been more in relation to the tests themselves, so there could be a flaw in the

test, and I hope to ask you to explain it to us, what I might call for shorthand, the "H9 problem". So that might be one issue with the test or a test.

But also I can imagine that there must be difficulties in deciding how one is going to set a level for a positive result. So what is it that is to be defined as a "positive result", and that must be difficult too. Am I missing something in my recital of the difficulties or --

A. No, both those statements, I think, are quite accurate.

Q. Thank you. Going back then to that statement in the MMWR, I think possibly I'm reading more into it than is meant by it, but that statement that in a population where the incidence of disease is low, the proportion of false positives will be high.

A. Yes.

Q. Well, what is your response to that?

A. Well, let me try and explain it as I understand it.

If you have a proportion of tests that registers positive, that are really false positive, that may be at a fairly fixed percentage. So to take the early version of the Abbott test for antibodies to HIV, there was a false positivity of somewhere around 2 to 3 per cent. Now, if you are screening blood donations from a volunteer donor population where less than 2 to

3 per cent of the donors are genuine positives, then the majority of the samples that register as positive will be false positives. If, on the other hand, you are screening a population where perhaps 25 per cent of the donors are genuine positives, as has occurred in some parts of Africa, then the number of -- the proportion of false positives is likely to be much lower.

Q. Yes.

A. Have I made myself clear?

Q. Yes, I understand -- I think the key is in the use of the word "proportion" actually.

A. Yes.

Q. I think, when I first read it, I was confused that the possibility of an absolute number of false positives would be high, because, of course, that's not what happened to you. When you used the test in the summer of 1984, which you had devised, in relation to the blood donors and from the Cheingsong-Popov piece -- I'm sorry, that's how we refer to it. That may not be accurate but I think just because it's the first name.

A. Yes, she was a young lady in my lab who developed this test in Professor Tedder's lab.

Q. Right. So we have been calling that paper by that name, and it was published in the Lancet on 1 September 1984, and I think from that there were 1042 ordinary blood

donors tested, and you didn't have any false positives in that group of people. Is that right?

A. No, we didn't. I think there are two reasons. The main one is that our test yielded almost no false positives, so the test format, the design of our diagnostic test, was not prone to false positives.

In my letter I have gone into some of the reasons why. Another reason, compared to the early statistics on the American test, is probably that at that time in the summer of 1984, the proportion of the British population who were donating blood, the proportion who were infected, was even smaller than in the volunteer population in the States.

Q. Yes.

A. And if you then include people who were paid to donate blood in the United States, then the population of positives went up. So we had a low risk of HIV contamination from donors. Sadly it was not a zero risk. And we had a test that didn't yield false positives.

Q. And I think I should ask you about that. We are going to call up your letter on our screen. I hope you have your own letter with you.

A. I do.

Q. So we can ask you about it. It's [PEN0171261].

The last paragraph on the first page explains about there being no false positives in the group of blood donors. You tell us that one of the reasons was -- and this you itemise as your second -- is the cell line. I'm doing this slightly out of order because I want to ask you about the cell line as well. But if you look at the third, you say that you were using a competition format, that is the donor's blood sample was titrated against a known positive antibody control, which the sample had to displace in order to register as positive.

I think we can understand the notion of a competition format in that the unknown sample, as it were, has to beat a known sample. Is that right, in the competition it has to win?

A. That's right.

Q. But I think it would be helpful if you could explain to us a little more how that works. How does the victory happen?

A. I'll try to explain and when my former colleagues, Philip Mortimer and Richard Tedder, give evidence, you may get a better explanation.

So in our case we had a known positive sample, and we looked for a positive sample where there was a high level, or titre, of antibody and where we could dilute it down to a certain standard, where it was diluted out

to have a lower level but still always registered positive in a direct test. And then that was used as a standard for all the tens of thousands of tests that could be made, or in our case at the research stage, a few thousand that we tested in this country and in Africa.

You then come in with your unknown blood donor sample, and if it is negative for antibodies to HIV, it should not affect the reading of the known positive standard. And in the case of radioimmunoassay you therefore get a reading of radio activity emission. In the case of an ELISA format, you would get a colour resulting from an enzyme reaction.

If that unknown blood donor sample was positive, it would dislodge the -- compete with the reaction of this highly diluted standard and so you would get less radioactivity or less colour from an enzyme test. So you pick out, amongst all the negative tests -- the little wells you are reading are all highly coloured, and you pick out the ones that don't have a colour or don't have radioactivity, and you say, "Ah-hah, what's going on there?" and those you count as being positive for antibody to HIV.

Q. Right.

A. I have probably confused you more than at the beginning.

Q. Well --
A. I'm sorry, but that's the way it works.
THE CHAIRMAN: Not at all. Could we just get a physical description of what titration is? Are you mixing samples in some way? What is titration in this context? You say when it's titrated against a known positive antibody control. What is happening?
A. Yes, you are.
THE CHAIRMAN: You are? Then I think I can see it.
A. You would be much better off asking Richard Tedder for the precise details.
THE CHAIRMAN: It is not the precise details --
A. Because I have not personally performed these tests.
THE CHAIRMAN: It's not the precise details at the moment, it's just a general picture. So one should understand that there is a control sample that just, as it were, gets over the hurdle and is registering positive, and then to that you are adding something that will only increase the level if it itself is infected and if it's negative it won't push anything up, and it changes the colour in the course of the process.
A. Yes.
THE CHAIRMAN: Right.
A. You are actually mixing the sample. In a direct test you would have a positive standard well, you would have

a negative standard well and you would look at all the test wells, but here you are actually mixing and dislodging.

THE CHAIRMAN: Ms Dunlop, is that what you would understand it to be or ...?

MS DUNLOP: I think the only term which I'm struggling with slightly is the term "dislodging".

In what sense is the first sample dislodged?

A. Well, these tests had components of the virus in them and that's why we will come on to cell components as well. These were prepared by growing the virus in human cell cultures and extracting -- making a cell extract which included viral proteins, and the antibodies in an infected person would attach to those proteins.

So you have a constant level of viral proteins which we call "antigens" and then you are asking the antibodies that are specific to the virus to bind to them. Where the competition and the dislodging comes in -- maybe "dislodging" is not the most accurate word I could have used, but the competition to come and bind to these proteins is that you have your standard known antibody, which in our case had the radioactive label on it, and you have your unknown sample and they are both competing to bind to the same virus proteins. If there is an excess of antibodies from the unknown donor

sample, if that donor was infected with HIV and had made antibodies, there would be so much there that there would be a much lower chance of the radioactive label standard antibody to be able to bind to the same virus proteins.

Q. Right. Okay. So it's a contest in which it succeeds because it's stronger, as it were, for want of a better term?

A. It's there in larger amounts.

Q. Yes, right. So it knocks out your control, which is --

A. Yes.

Q. -- deliberately weak but a known positive?

A. Yes, where I'm not quite sure is whether the standard one was already lodged, in which case it is dislodging. I think I have probably used an inaccurate term there. You are just adding the two together and they are competing to bind at the same time.

Q. Is it then correct to think of that concept, the competition concept, as being available to somebody making an ELISA? So you can design an ELISA --

A. Exactly the same principle.

Q. Yes, right.

A. Yes.

Q. I'm jumping about a bit here, I'm sorry, but do you know, did that feature continue to be present in the

Wellcozyme test, the test that was finally developed by Wellcome in 1985?

A. Yes, the first generation tests that were manufactured and sold used that competition format.

Q. Right. Actually you do cover that in your letter, I think, because you say that that was the big discussion in early 1985.

Before we go to that, can I ask you then about the second point you make in the same paragraph, that:

"The cell line CEM, in which we propagated HIV-I to produce antigen, was different from the US cell line."

This is almost impossibly crude but I have been thinking of the cell line as analogous to the role of compost; you need something in which to grow your virus? Is that right? Yes?

A. Yes, but it's a little bit different from growing plants in compost. A virus is a very small parasite that has to get inside the living cell. So HIV, and Hep C for that matter, can only propagate themselves by getting inside human cells. In the case of HIV, inside white blood cells, in the case of HCV, inside liver cells and some white blood cells. So they are getting inside, they are then taking over the machinery of the human cell to make more of themselves. They are what we call an intracellular parasite.

Q. Again, this is back to the Crewdson book, but there is a description of the problem that manifested itself with the Gallo cell line, which I was referring to for shorthand as the "H9 problem". That was responsible in its own right for false positive results, as I understand it, with some of the American tests. Is that correct?

A. That's right.

Q. Perhaps you could explain that a little bit to us.

A. The H9 subline of the leukaemia cell line that it had been devised from was a very useful type of cell for propagating HIV. You could grow large amounts in it. The problem, with hindsight, when they adopted this means of propagation of HIV proteins or antigens, was that there were certain human proteins that also elicit antibodies in other humans. So there would be a higher rate of false positive tests because the viral proteins were not separated properly from the human proteins. These human proteins are embedded in the lipid membrane of the cell and the virus, HIV, when it is released from the cell it has been growing in, takes that lipid envelope with it. So there are not only virus-specific proteins there, there are some host proteins as well.

There was a particular type of protein, called the MAT Class II for short, to which some people have

antibodies to other people's variants of those proteins. They are the same proteins that prevent organ transplantation taking unless you are put on immuno-suppressive drugs. So we reject each other's cells or organs.

And H9 expressed large amounts of one particular kind of MAT antigen called DR4. And people can make -- normal healthy people may make antibodies to DR4, in particular, as Crewdson writes, but it's on page 190, not on the two-page excerpt here.

Q. Yes, we can get that.

A. Women who are married to DR4-positive husbands, if they have several children, they are likely to make antibodies to DR4. And there are other reasons why you might make them. You may have had a blood transfusion yourself. You may have received platelets. And then you are likely to make antibodies.

These can give that background of false positives that would not occur if the viral proteins were made in a cell line that did not have these proteins, and that is why we used the CEM cell line and so did the French, the Institut Pasteur.

Q. We are just going to look at that --

A. They licenced them from us.

Q. We are just going to look at the section. I'm sorry, we

didn't send you the right page but it's exactly as you say. At the bottom of 189:

"The H9 phenomenon was first noticed in Germany where a surprising number of middle-aged women had begun testing positive for the AIDS virus."

Then you are quoted as having said:

"Staid matrons who had only ever been married to one man ..."

Said Robin Weiss:

"They turned out to have the same HLA group, these women, as H9 cells."

A. That's another error in Crewdson's book. He gets to it in the second half of the sentence. It's their husbands who had the same, and therefore they didn't have it and therefore they made antibodies to it.

Q. Okay. Then they said there was a problem with thousands of Americans, including a group of black farmers in rural South Carolina. Is he right about that, that they had a false positive rate of 300 per cent?

A. That means there were three false positives for every genuine positive.

Q. But your cell line was free of this particular difficulty, and you say the French one was too --

A. Yes, and we were advised to use it even before we knew of this difficulty, by a colleague,

Professor Mel Greaves, who felt that this could be a complication, before we knew that it was.

Q. Right. And in fact, that complication applied to all five of those licensed in the United States, the five companies who had been licenced, because they were all working from the HTLV-III B isolate, as I understand it. Is that correct?

A. The five companies that were licensed to develop diagnostic tests from Dr Gallo's research at NIH all had the same supply of viral proteins grown in H9 cells, but there were independent isolates in the United States. For example, Dr Jay Levy in San Francisco, where another company started developing tests, and as that was not subject to control by the American Government, they were free to format it in different ways.

Q. Right.

A. But they were behind hand in coming to the market.

Q. Yes. I think that does take us to the third question, which relates to France and the French research. We asked you:

"Given that you had obtained LAV from Paris earlier than you had obtained the Gallo isolate, why was your RIA not developed from the French isolate?"

Then we also asked about general availability in the UK of the French test. Are you able to give us some

information about those points, professor?

A. Well, if I take the first point, I don't have any good reason why we happened to develop the initial RIA with the isolate from NIH, rather than the isolate from the Institut Pasteur. We did indeed receive the virus from Paris earlier and we had it growing successfully earlier, and we provided it back to our French colleagues, growing in CEM cells, because they themselves had never succeeded in growing it to high titres, to high levels, because they were using fresh blood cells each time, and they used that for their Elavia test.

I think we just happened to be growing both isolates and provided Gallo's isolate to our colleagues, Richard Tedder and colleagues, to do the labelling of the antigen, and we could have just as well chosen the French one. They were growing to more or less equal levels, if I recall, but you have to choose one or the other.

Q. I appreciate it may just have been happenstance, but if you had developed the RIA from French virus, the intellectual property problems, the licensing problems and so on that were encountered with the Americans might not have occurred.

A. I don't really agree with that statement.

Q. It was a question, sorry. I was wondering if you thought --

A. Okay, well, I didn't see a licensing problem myself. The only intellectual property that we protected was the format of the competition test.

Q. Right.

A. And no one challenged that patent. We obtained the French isolate and three months later, when we obtained the American isolate, we assigned what are known as MTAs, Material Transfer Agreements, which are common documents between research labs that say you can have this for research but (a), the provider accepts no liability, (b), you must not use it for commercial developments and so on. These are fairly standard documents. And we signed with both sources of virus.

When it was decided that Wellcome Diagnostics Limited should develop a test based on our protected competition ELISA technique, we provided an isolate that we considered was one of our own and therefore was subject neither to the French or to the American protection.

Q. Right.

A. And we had already made these and -- so to my mind there wasn't a big delay in Wellcome Diagnostics Limited developing our test. They were never going to do it

with either the American or the French one but with one of ours.

Q. Right. I think the perception that we may have had is that the refusal by the Department of Health and Human Services in Washington to allow the first RIA to be scaled up for general use in the NHS, that refusal was productive of delay because you then had to start and develop your own isolate, but you are telling us that isn't quite how it was?

A. We didn't start to develop our own isolate when we heard this news, we had already developed an isolate.

Q. Right, thank you.

A. And we provided one of them.

Q. Yes.

A. But I'm sure it's correct, though I don't recollect these events, that if there was discussion whether a non-profit-making test could be provided to the NHS, that we might have wanted to go straight in with the one we had done our initial research on. That happened, as you said, happenstance, to be the American isolate rather than the French.

Q. Right.

A. In fact the American one was the French one but that didn't become clear until several years later.

Q. Yes.

THE CHAIRMAN: That's the puddle we have been trying to avoid.

MS DUNLOP: I don't know if you saw this, professor, I don't think we have sent you this section but we do have one section in our preliminary report saying:

"Controversy over the discovery and its attribution did develop but is not of immediate relevance to our Inquiry."

And we are trying to hold to that line, although it's very interesting and --

A. It was enormously controversial but to my mind the simple facts of precedence are crystal clear. The French published a paper that turned out to be correct in May 1983. The first American paper from any American lab was in May 1984. There were 11 and a half months' difference in time. And just as in filing patents, if you are putting a scientific find into the public domain, it's whether you have priority.

So to me it was a no-brainer. The French discovered the virus, full stop -- or "period" as the Americans would say.

Q. I think we may have accepted at an earlier stage something roughly along the following lines, that the French didn't realise what they had found and it was really the Americans who did the important work in 1984

by pointing out the significance of this virus, but I don't think that's really accurate either, is it?

A. No, I don't think it is. I think it took all of us in the field a long time to agree that the French results were standing the test of time. So, as a confirmation, the American studies were very convincing. So the world woke up and said, "Yes, this is it", but it was a confirmation.

I could have sent you a paper I wrote when the Nobel Prizes were awarded in 2008, where the Americans lost out, where I explained and cited papers that showed that the French actually had four papers, not just their first one but three subsequent ones before the first American one appeared. I'm not a particular French chauvinist. I did do a sabbatical in that institute later on. But I am a stickler for the published record in research papers. And being emeritus I actually have time to go back and read them, and that was the situation.

Q. I think if you are able to send that to us, we would very much appreciate it.

THE CHAIRMAN: Thank you, Ms Dunlop. I didn't know that I could give in to the temptation.

MS DUNLOP: We would, I'm sure, like to read it. So if you are able to do that for us.

THE CHAIRMAN: Very much so.
A. I'm actually on my way to Gatwick airport. Can I do it late next week?
MS DUNLOP: Absolutely. I didn't mean immediately by any stretch of the imagination. I know you are in fact going on holiday. The last thing we would want would be to divert you from that.
A. I will forward a PDF to Lindsay.
Q. Thank you very much.

We did also ask you about a report that we had discovered of a visit by a group including Dr Crawford to Professor Tedder's laboratory in 1984. I think you should have a hard copy. It's [SNB0048803].

I don't want to take up a lot of time with this and we can certainly ask Dr Tedder about it, but was there anything that you identified in this report that you wanted to comment on?
A. No, I don't think there is. It's a detailed quantitative method on the source of antigen -- that is the virus proteins. It's talking about safety and how these proteins are prepared, and I regret that I don't recall who Dr Crawford is or was. I don't think I knew him at all well personally.
Q. No. He is from the transfusion centre for the West of Scotland.

A. Oh.

Q. And we have come across him on a number of occasions in our references.

I also wanted to correct something that we had said in the preliminary report, and this is our question 7. We did say in the preliminary report that we were unaware of anyone who acquired AIDS in Scotland from transfusion between March and October 1985, but we are now aware that there is at least one person and I wondered if that altered what you had said in your letter. We should have that in front of us, page 3 of [PEN0171261], please.

A. Yes. I was asked, as stated in the letter under (b):

"Would it have been preferable perhaps, in hindsight, for the UK to introduce one of the commercially available tests in 1985, even on a short-term basis, rather than await the results of an evolutionary programme."

This is an important part of your Inquiry and I'm going to cop out. I sit on the fence on this. First of all, I don't think there was a commercially available test in March 1985, and as I said in my letter, I don't think that sufficient supplies of tests could have been commercially available before the end of May 1985, rather than March. But I might be mistaken.

But you could say, "Well, surely it had been introduced at the end of May?" I'm not even sure then that the companies that had won these contracts for the United States were in a position to supply the UK.

They were certainly very keen to have a monopoly on these tests. The French seemed to be behind hand, Wellcome Diagnostics wasn't quite ready yet. The other tests in the States that were independent of Dr Gallo's isolates and Dr Gallo's H9 cells were not yet on the market.

So I think one would have to probe when would these tests have been genuinely available to the UK. Were the batches that could have been available to the UK approved by the FDA, because there was discussion at EAGA: does Britain need FDA approval? Which is the United States agency. On the other hand, if the UK went for FDA non-approved batches, maybe those were the poor quality batches that were just being dumped on an overseas market. There were a lot of implications then, and I personally don't know the answers.

The question perhaps you are really asking me is: assuming that commercially available tests, that were good tests, were available earlier in 1985, should they have been used? And I suppose that if genuinely reliable tests, that had a low enough false positive

rate that they did not mean that most batches of clotting factors would have to be discarded, that there were too many individual volunteer donors' blood who would be discarded, that people wouldn't be told they are infected with HIV when they weren't -- if that false positivity level was low enough, I would say, with hindsight, anything that prevented patients or people treated with blood or blood products, that would prevent them getting this deadly virus, those measures should have been taken. In those days, it was a largely lethal infection in the long-term and it was a terrible thing to become infected with HIV.

As I say in my final comment, we were handling large quantities of live virus ourselves and we didn't know even how dangerous it was to handle in the laboratory. We tested ourselves before there were commercial tests with our own homemade test every month because if one of us became infected we didn't want to pass it on to our partners, our husbands and wives. But at that stage in 1984/1985, we were groping at the edge of this alarming new disease and there weren't clear answers to clear questions.

Q. Thank you.

THE CHAIRMAN: Professor, I'm terribly anxious not to proceed on the basis of a hypothesis that can't be

validated. Do you consider that there is any possibility of establishing positively the essential elements in the hypothesis you have set out, assuming that --

A. I'm sorry, can you speak a little closer to the microphone? I didn't catch the first half.

THE CHAIRMAN: Is there any possibility of validating the hypothesis in which you developed that answer? It seems to assume that there would have been what would have been accepted to be generally available, generally reliable tests. Can that be established?

A. Erm, it might be worth examining the EAGA minutes in detail. Perhaps the Inquiry has already done that. Because there was a lot of discussion but, as I have said in my letter, I'm not sure how much was coffee-break discussion and how much was minuted discussion. There may be documents in the United States as well but one would have to subpoena them from the companies and it would not be easy to get hold of them.

I guess -- my hypothesis -- it's not exactly a hypothesis. I'm saying I'm somewhat sceptical that there were enough good reliable tests to supply the UK. I don't think there were in March 1985 and I'm not certain they were in May 1985.

THE CHAIRMAN: Well, if anyone thinks they know, perhaps

they will let me know, Professor Weiss.

MS DUNLOP: I'm not sure, professor, that we really understand what happened with the French test. You said it was behind --

A. I don't know what happened and I haven't looked up the history. We did have some slight difficulty with the French because they signed an agreement to cross-licence from us the process of using CEM cells. They then derived their own subline of CEM cells from the American-type culture collection and ignored this cross-licensing, and I had to -- or my institute's lawyers had to point that out to the lawyers for the Institut Pasteur.

At the same time we found that our so-called first British isolate was yet again a contaminating virus from the French one. The French had even contaminated their own, I might say, and we cut through the lawyers. I shouldn't say that to a bunch of eminent lawyers listening to me now but we proposed that since both the Institut Pasteur and the Institute of Cancer Research had unwittingly and unintentionally erred, that we would simply let bygones be bygones and not sue each other for using each other's materials. And that was signed within 24 hours with no extra fees.

PROFESSOR JAMES: Professor Weiss, I can tell you there are

some very black looks in this room that the lawyers didn't get any fees out of this.

THE CHAIRMAN: Not from me, Professor Weiss. It's a long time since I got fees out of anything.

MS DUNLOP: Professor, with my eye on the clock, I just want to ask you one or two short questions.

Firstly, I think we did ask you about what has been termed a "secret meeting". I think this must have been towards the end of 1984, involving possibly somebody from the Department of Health and yourself and Professor Tedder and someone from Wellcome. Do you remember any particular meeting at which the way forward with the test was discussed?

A. I'm sorry, I don't remember. I may have been there but there were also meetings when I wasn't present between Professor Tedder and the department and Wellcome, because I was a little bit on the edge of things.

Q. Right.

A. I may have been present but I don't remember this meeting.

Q. Right. And were you involved in designing the evaluation protocol for the evaluation exercise in 1985?

A. I was a member of EAGA but I wasn't on the subcommittee that developed that. There is a statement in your preliminary report that Professor Tedder and I applied

to do this. My recollection is that we were asked whether we might do this because we were the only lab that was routinely doing these kinds of tests that was in a position to evaluate them, and we ourselves felt that this was something of a conflict of interest.

So we rather reluctantly put in what we would do if we had to do it. We were willing to do it rather than it not be done, but we were delighted in the end that the Public Health Laboratory Service, as the HPA was then called, and Philip Mortimer's lab, took it on.

It was much better that that was done, and the only contribution I think we played was to help train one or possibly two members of personnel from Philip Mortimer's lab in how to handle this virus, as they hadn't previously handled it.

Q. Yes. You do cover this on the second page of your letter, if we just call that up again, [PEN0171261].

You say you dropped out of both activities, that is developing a test and evaluating the tests. This is towards the top of the page. You dropped out of both activities once Wellcome Diagnostics Limited had been passed the reagents, and you explain that you carried on work in relation to disease in Africa.

A. Yes, I didn't turn my back on it. I appreciated even then how extremely important that was for public health

but my skills were in discovery and exploring new areas. So we discovered the receptor for HIV, the CD4 protein. We, as I said in this letter, were beginning to -- we had already turned our attention to Africa and that's why I felt I could make the strongest contribution to this dreadful new disease.

Q. Yes. You also explain, helpfully for us, on this page at the bottom about some of the reasons why the ELISA format was preferable to the RIA format. I think we understand from your explanation what those reasons would be.

A. Again, I think the other expert witnesses on tests might have a better recall of when test generally shifted from RIAs to ELISAs, like the pregnancy test, for example. But I think there were obvious advantages so long as it performed as well as the RIA test.

Q. Right. Excuses me a moment. (Pause)

Thank you very much, Professor Weiss. If you could hang on a couple of minutes until we ascertain if anyone else wants to ask any questions.

A. Certainly.

THE CHAIRMAN: Mr Di Rollo?

MR DI ROLLO: No, thank you, sir.

MR ANDERSON: No, thank you.

THE CHAIRMAN: Professor Weiss, I think that you have

conquered all.

Thank you very much indeed for your help and I'm obliged that you have managed to fit us in.

A. Thank you for arranging it when I was here.

MS DUNLOP: Thank you.

A. Thank you.

THE CHAIRMAN: Now, Professor Cash.

PROFESSOR JOHN CASH (continued)

MS DUNLOP: Sir, I don't have any further questions for Professor Cash but I think Mr Di Rollo has one or two.

THE CHAIRMAN: One or two, Mr Di Rollo?

MR DI ROLLO: Yes, one or two.

THE CHAIRMAN: What about you, Mr Anderson?

MR ANDERSON: No, sir.

THE CHAIRMAN: And Mr Johnston?

MR JOHNSTON: No, sir, I don't have any either.

THE CHAIRMAN: Unless provoked by Mr Di Rollo.

MR JOHNSTON: Yes, sir.

Questions by MR DI ROLLO

THE CHAIRMAN: Well, Professor Cash, welcome back.

Ms Dunlop has discharged you from any further obligation to answer her questions but Mr Di Rollo has some for you.

MR DI ROLLO: Professor Cash, I just really want to ask you about one particular matter and it concerns the issue of

it being indicated to SNBTS by, you say, the SHHD that evaluation would have to take place down south and that the SNBTS proposal to go it alone wouldn't do.

What I would like to ask you is if you had been permitted to go your own way, as it were, would the outcome have been different in the sense that it would have been possible to have tested or put in place screening earlier than October 1985?

A. I would have to say yes to that; we could have done it. It would have been a lot of work.

In relation to the thing I have just seen with Robin Weiss, there is a table, sir, in one of the documents that the SNBTS has got, indicating, country by country, when did they introduce screening for all sorts of different things and HIV is one of them, and you will see there, I'm fairly sure -- it's a document produced by Dr Prowse and I think Brian Dow together -- and there you will see that I think Australia, the Netherlands and France introduced full-scale routine HIV screening in May -- April/May 1985. And I take a simple view: if the Aussies can do it -- one needs to know what they got up to before, though -- and the French -- in terms of assessment. (a), that must have a lot of kits that were being made available at that time. I have no idea where people have got it from. (b) they must have done a lot

of background work, and Pim van Aken will be here again soon, I think, and he will be able to explain what happened in the Netherlands.

But they started, we know, in that period of time, and I think we could have done that if we had had the appropriate push, yes. Whether it would have been significant clinically, I really don't know. I think you have to ask the question which you have been over in previous sessions: where were we in terms of heat treatment in January 1985?

Well, I'll tell you where we were; we were issuing it for Factor VIII to the haemophiliacs like there was no tomorrow, so that was wonderful. If you ask Peter Foster, were we absolutely certain that the heat that you were giving these products really was knocking HIV on the head, he will say to you, "No, we weren't".

And therefore, you can argue that if we had started that bit earlier -- I mean, I was terrified that we had HIV now in the Scottish donor population, and I feel that the odds were on we might, in the event, have done some value by starting earlier.

Peter may now reflect, and rightly so, that actually now he knows the heat treatment that they first introduced for our Factor VIII and IX wasn't any good, I think, for HCV but was excellent for HIV. So I can't

answer your question really.

Q. Your answer that you have given is taking into account obviously, the issues in relation to all the other things that would have to be sorted out, such as counselling and alternative screening venues.

A. Yes.

Q. -- and all these other things?

A. Absolutely. Big job.

Q. But you think that that all could have been dealt with and put in place before October if SNBTS had been given --

A. Yes, I do, knowing our team, I do.

Q. What sort of timescale are you talking about? Are you talking about months in terms of --

A. I find it very difficult after all these years but it wouldn't have surprised me that we could have been alongside the Dutch and the Australians.

Q. All right, thank you.

That's all I have, sir.

THE CHAIRMAN: Are you still content, Mr Anderson?

MR ANDERSON: Yes, thank you, sir.

THE CHAIRMAN: Mr Johnston?

MR JOHNSTON: I have no questions, sir.

THE CHAIRMAN: Ms Dunlop?

MS DUNLOP: I have no further questions, sir.

THE CHAIRMAN: Professor Cash, thank you very much.

Yes, Ms Dunlop?

MS DUNLOP: No further witnesses for today, sir.

THE CHAIRMAN: Thank you.

(4.15 pm)

(The Inquiry adjourned until 9.30 am the following day)

I N D E X